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I. THE BALANCE OF NATURE.*

WHAT naturalist does not look back to his first perusal of Waterton's works as one of the most agreeable reminiscences of his boyhood? Even now, notwithstanding the wonderful transformation which Natural History has undergone, the "Wanderings" and the "Essays" retain their old charm unimpaired. If we attempt to analyse the merits of Waterton we cannot, indeed, regard him as a systematist. His chief attempt in that direction, the arrangement of the Quadrumana, formerly so called, into four groups,—the tail-less, or apes; the short-tailed, or baboons; the ordinary long-tailed monkeys of both hemispheres; and, lastly, the prehensile-tailed species of South America,—might almost be regarded as a satire on the classifications of some of the older naturalists based upon a consideration of some single organ. If taken seriously, it has the striking demerit of breaking up the platyrrhine family into two groups, in defiance alike of morphological and of geographical considerations. But if unable or unwilling to inaugurate a "system" of his own, Waterton's clear incisive common sense refused to accept the Swainsonian doctrines, which reigned supreme during many years of his life. "Who knows," says he, "but that some closet-naturalist may account for these alar spurs of the camichi through the medium of that very useful and important discovery, the quinary system. Thus, for example, suppose these said spurs were once normal or typical on the legs; but by some rather obscure process, having become aberrant, they

* Game Preservers and Bird Preservers. By G. F. MORANT. London: Longmans and Co. Essays on Natural History. By CHARLES WATERTON. Edited by NORMAN MOORE, B.A. London: F. Warne and Co.

made an approach or passage to the wings; whilst the bird itself was progressing in the circle or leading round, in order to inosculate with the posteriors of its antecedent. He who clearly understands the quinary system will readily understand this." Scientific nomenclature he avoided, and even denounced as abstruse, complicated, and incomprehensible—a view likely enough to be taken up by one of the working-class naturalists who used to be met with in the North, but strange and scarcely consistent in a gentleman who had received a classical education, and who was rather too much given to Latin quotations. Had he lived in our days this prejudice for a vernacular terminology might have been a pardonable and even useful protest. Generalisation was not his department. Those questions concerning the origin, the modifications, and the distribution of species, which are agitating the zoologists and botanists of our time, and which must be definitely answered before Natural History can be formally ranked as a Science, do not appear ever to have presented themselves to his mind. He accepted every bird and beast as an ultimate fact; he scrutinised its structure, noted its habits, but never enquired why it was found in one country and not in another, nor what were its relations to other species, co-existent or extinct. Neither can Waterton be regarded as free from prejudices. He stoutly upholds the conventional doctrine of a great gulf between man and the lower animals—a distinction not of degree, but of kind. He denies reason to dogs, to foxes, to apes, and, in short, to all the lower animals, and regards it as a characteristic of man alone. All instances of rationality on the part of bird or beast he regards with an excessive scepticism, somewhat singular in a man who could record his firm belief in the miraculous liquefaction of the blood of St. Januarius, at Naples. He entertained the view that no carnivorous animal could be gregarious, and hence was led to deny that wolves and other Canidæ were naturally in the habit of hunting in packs, and of acting in concert to secure their prey. It is needless to say that had he ever visited Russia, or the more northern parts of North America, he would soon have found overwhelming evidence to the contrary, and might easily have been convinced of his mistake by his own observations. In addition to the dog tribe, the common weasel has been known to run in packs, and men have occasionally been hard set to escape with their lives from the pertinacious attack of a band of these little vermin.

Waterton also denies that serpents, unless trodden upon

or intercepted in making their escape, ever act upon the offensive towards man. In his extensive experience he had doubtless never met with such a case. Our own observations agree in this respect with his, but for all this we are not prepared to deny that the ophiophagous cobra—a specimen of which is now to be seen alive in the Zoological Gardens—will, under certain circumstances, attack passers-by. Waterton's great error was a readiness to assume that what he had never seen, or perhaps never had had the opportunity of seeing, must be imaginary. But if, as we have good reason to believe, he is wrong in this particular, he has made ample amends by exposing and refuting a multitude of idle stories about serpents which are related even in books of some pretensions to scientific accuracy. As regards our English serpents, Waterton seems to apply the name of "adder" to the common "water-snake" (*Natrix torquata*), but he underrates the venom of the viper: death has undoubtedly resulted from its bite, even in England.

As a controversialist Waterton was truly formidable, if somewhat too outspoken for modern tastes. Any error in the writings of an opponent he was sure to detect, and to expose without mercy. Witness his treatment of Audubon's rattlesnake, figured with its poison-fangs curved the wrong way. Upon pretentious dabblers in Natural History, who seek to force themselves into notoriety by retailing the results and sometimes the blunders of others, he was severe. He "scourged two generations of quacks," and we sometimes think he might do good service, even in the present day, could he be roused up from his slumbers beneath the old oaks in Walton Park.

We never heard of his being connected with any scientific society; perhaps he might feel unwilling to place himself upon a level with "Diabolus Gander," and others of his contemporaries who were Fellows of every existing society, and would doubtless have been "Fellows" of Vauxhall and Ranelagh had those establishments been original enough to assume a semi-scientific disguise. His paramount merit is as an observer of phenomena, and in this department he has had few equals and no superior. In order to see with his own eyes he deemed no trouble or danger too great, and whatsoever he saw he describes faithfully and clearly, overlooking nothing and exaggerating nothing. The great misfortune is that all his writings have not seen the light, and that he committed to paper not one-tenth part of the facts that he observed. He has left us, as it were, merely a sample of his labours.

It is not, however, our present purpose to enter upon an exhaustive appreciation of Waterton as a naturalist, or to decide upon his ultimate position in the history of science. We have to consider his views upon a subject which has embarrassed naturalists and teleologists almost as much as the "balance of power" has perplexed politicians. Is there a balance of Nature? If so, in what does it consist, what does it signify, and in how far can it be modified to our advantage? These are the questions we have to consider, and it will be at once conceded that they not merely concern the zoologist or the botanist in his study, but come home more or less to us all in the concerns of daily life.

If we examine some island, as yet untrodden or rarely visited by man, and take a broad view of its fauna and flora, we must admit that a certain balance exists. The species there found, by the very fact of their co-existence, are proved to be mutually compatible. The carnivorous mammal, bird, or insect does of course devour a number of its feebler fellow-denizens of the island: the phytophagous mammal, bird, or insect, in like manner, preys upon the plants. But the greater fecundity of the species preyed upon, or some other attribute, preserves them from extirpation, and even prevents any appreciable decrease in their numbers. The equilibrium may not, indeed, be absolutely perfect. Could we visit such a region at successive periods of a century each, and take an exact census of its population, animal or vegetable, we might find this or the other species growing gradually rarer, and finally becoming altogether extinct, whilst other species, on the contrary, were slowly but surely increasing. Such changes may arise not merely from the ravages of the destroyer slowly gaining ground upon the fecundity of some one kind of his prey, but still more decidedly from changes of climate. However, within moderate periods of time, the equilibrium in any country may be regarded as complete until the intervention of man.

Recognising, then, a certain phenomenon which may be called the balance of Nature, we have to consider its origin and meaning. On this subject there exist various theories pointing the way to diversities in practice.

The view formerly most general may be considered a corollary to the Miltonic interpretation of the Mosaic cosmogony.* It regards the fauna and flora of every region, as especially selected by Providence, as perfectly adapted

* It is significant that Prof. Huxley should be accused of insulting the understanding of the American people by having referred to this poetical interpretation as still accepted in certain quarters.

both to each other and to the soil and the climate of their locality. Writers holding this opinion have even likened the various animal and vegetable species to the wheels and pinions of some delicate and complicated piece of machinery, none of which can be removed without injury to the whole. Man's true policy, therefore, is to let Nature alone. The more he interferes the less reason will he have to be satisfied with the results. It must be owned that far too many facts lend plausibility to this view. In nothing has man shown a more glaring want of prudence than in the wars he has waged against the lower animals; but he cannot, however willing, leave the balance of Nature undisturbed: he is compelled, by the law of self-preservation, to extirpate such carnivorous, venomous, parasitical, or otherwise destructive species as are dangerous to himself, his cattle, or his crops. Singularly enough, his appliances for waging war against such creatures have been developed far less rapidly than his means for the destruction of his fellow-men. But this is not all: he brings with him, in some cases by design and in others unintentionally, animals and plants which soon exert a powerful modifying influence. He is almost invariably accompanied by the grey rat, which at once goes to work to revolutionise the indigenous fauna. The small birds which have hitherto built in security on the ground, or in low trees, have their eggs and young carried off and devoured; the lizards and the terrestrial Mollusca are attacked on the land, and the smaller fishes in the streams. Apterous insects, and the pupæ and larvæ of winged kinds, are sought for in all their haunts by the energetic destroyer; and entire species are thus erased from the muster-roll of Nature. Such is the inevitable result where the rat is kept in check neither by serpents, birds of prey, nor carnivorous beasts. As an instance we may take Mauritius, where rats have multiplied to a frightful extent, and where the present poverty of the fauna, especially in insects, is notorious. But the animals which man introduces intentionally, and for his own use, are perhaps equally destructive. Foremost in this respect stands the goat; he browses away the shrubs and the young seedling trees, and thus ultimately extirpates the woods. With the trees perish the insects; and with the insects, fruits, and seeds the birds disappear also: the destruction of the aboriginal vegetation of St. Helena—a loss deeply felt by all students of organic geography—must be, in the main, ascribed to the ravages of goats. Swine take a prominent part in the work of destruction, and are particularly busy in eradicating all vegetables with bulbous or tuberous roots, or

with succulent stems and leaves. Scarcely less formidable is the rabbit, which, when thoroughly established in a region free from beasts and birds of prey, renders the introduction of agriculture almost impossible. To naturalists, at least, it is a subject of grave regret that a project has been brought forward for stocking uninhabited islands with goats and rabbits, under the pretext of thus providing a supply of food for shipwrecked sailors. The result of this scheme, if carried into execution, will be the destruction of much priceless evidence bearing on the distribution of animal and vegetable life, and hence indirectly on the great question of the origin of species.

But worse still remains: the islands of New Zealand are suffering under a grievous plague of rabbits. It has been suggested by an "energetic and indefatigable naturalist" to introduce, by way of a remedy, polecats! Suppose this done, it is very probable indeed that much havoc may take place among the rabbits; but is it not likely that the poultry of the colonists may receive certain unpleasant attentions, and that some of the few native birds will be exterminated? Nor is the action of the animals imported by man the only agent of change. The aboriginal plants, contrary to the theory of their especial and exceptional adaptation to the soil and the climate, largely succumb to the species introduced by the settlers, many of which, unfortunately, are weeds of the most noxious character. Thus we see that the arrival of man in a previously unpeopled region completely disturbs its original "balance of Nature," even if he never discharges a gun. But as, in addition to the havoc thus indirectly occasioned, birds good for food, or suspected—rightly or wrongly—of being injurious to crops, are diligently shot at, we need not wonder that the old equilibrium is speedily overturned. A different theory is therefore forced upon all candid thinkers. Whether they are believers in original and independent creation, or in evolution, they must equally admit that the old "balance of Nature" is merely provisional, suited to man's absence, but incapable of being maintained in his presence. A new equilibrium has therefore, if possible, to be established, where all species of animals and plants dangerous or hurtful to man's person and possessions shall be exterminated, or at least very much reduced in numbers, whilst those which are useful, beautiful, or otherwise agreeable to the lords of the creation, shall be protected and encouraged. That such a state of things would be theoretically desirable we shall all agree, but how is it to be realised in practice? Too many farmers,

colonists, &c., destroy the wild birds upon very insufficient evidence, and then make, too late, the unpleasant discovery that they have been extirpating their best friends. Thus, as Waterton tells us, "the North-American colonists got the notion into their heads that the purple grakle was a great consumer of their maize, and these wise men of the west actually offered a reward of threepence for the killed dozen of the plunderers. This tempting boon soon caused the country to be thinned of grakles, and then myriads of insects appeared, to put the good people in mind of the former plagues of Egypt. They damaged the grass to such a fearful extent that in 1749 the rash colonists were obliged to procure hay from Pennsylvania, and even from England. Buffon mentions that grakles were brought from India to Bourbon to exterminate the grasshoppers. The colonists, seeing these birds busy in the new-sown fields, fancied that they were searching for grain, and instantly gave the alarm. The poor grakles were proscribed by Government, and in two hours after the sentence was passed not a grakle remained in the island. The grasshoppers again got the ascendancy, and then the deluded islanders began to mourn for the loss of their grakles. The governor procured four of these birds from India, about eight years after their proscription, and the State took charge of their preservation."

The random destruction of wild birds has occasioned most serious results in France. Till a quite recent date every species of bird in that country was subject to persecution. The robin and the swallow were shot and eaten as eagerly as the quail or the ortolan. To make matters worse, our contingent of swallows take the route through France on their southward flight in the autumn, and are too often intercepted on their way. The Italians, in like manner, prey upon birds of passage, to the great detriment of the German and Swiss farmers. It is even intimated that negociations on this subject have been begun between the German and the Italian Governments. The consequence of this reckless assassination of small insectivorous birds has been a frightful increase of caterpillars, aphides, cockchafers, weevils, and other insect scourges of the farm and the garden. Worse still, blood-sucking flies have multiplied, whose bite is sometimes followed by carbuncle, and even proves mortal—possibly in cases when the fly has been previously feasting upon carrion in some particular stage of decomposition. The French have therefore perceived the error of their ways. Not only have laws been enacted to restrain the murderous propensities of misguided sportsmen,

but official notices have been posted up throughout the country, by order of the Minister of Agriculture, pointing out what birds, insects, &c., are injurious and should be destroyed, and what are useful and should be spared.

Perhaps Waterton's highest claim upon our esteem and gratitude must be based upon his energetic protest against such indiscriminate slaughter as we have just mentioned. Though a most humane and kindly man, he spoke not as a hysterical humanitarian, ever seeking to protect evil-doers, whether brute or human, rats or garotters, from the well-merited consequences of their misdeeds. His protest was uttered in the name of common sense no less than in that of compassion. He had, during a long and active life, devoted almost exclusively and under exceptionally favourable circumstances to observation, carefully examined the structure and the habits of birds and of certain mammals, and had noted the kinds of food which they select. His park was simply an ornithological "station," where all kinds of British birds were as far as possible protected, in order that they might be closely and accurately studied. Hence he was enabled to pronounce with authority as to what species were to be viewed as man's friends, and which were to be ranked as enemies. Not a few more recent British naturalists—among whom honourable mention is due to the Rev. Messrs. F. O. Morris and Tristram—have followed in his footsteps; and if our Legislature has been induced to extend any measure of protection to wild birds not in the game-list, to Waterton and to his disciples belongs the credit.

If asked, however, to describe in a few words Waterton's system of establishing a "balance of Nature" suitable for inhabited countries, we should reply that he sought to minimise the amount of man's interference. He would not sign the death-warrant of any creature till positively proved to be injurious; and if the evidence was imperfect, then, in accordance with the old-established practice of English justice, but in flat opposition to the custom of English gamekeepers, he would give the accused the benefit of the doubt. More than this, if—an exceedingly common case—any species were found to be in some respects harmful, but beneficial in others, he would endeavour to balance the evil against the good. Here, of course, there is room for a diversity of opinion among men of different views, interests, and prejudices. Suppose, for instance, that some particular bird frees our fields from millions of destructive insects, but at the same time occasionally carries off the eggs of the

pheasant or the partridge; what is to be the verdict? One man—thinking the food of the majority of the nation and the prosperity of our farmers a matter of greater importance than the production of costly delicacies, and than the amusements of a small minority—may vote for an acquittal. Others, such as Mr. Morant, may take the opposite view. It is certain, indeed, that Waterton may have been somewhat too lenient, and have overshot the mark, in his general protection of birds. It is the common, perhaps the inevitable, error of the reformer combatting some wide-spread and old-established error, that he goes too far; but he is doubtless much nearer the truth than any of his gainsayers, past or present.

The strangest view of man's proper policy towards the lower animals has been put forward by Mr. Morant. This author has certainly some points of resemblance with Waterton. He has travelled extensively; he has "pursued and collected birds over a great part of India, and for some years in South Africa." Hence he may, perhaps, be regarded as a compound of sportsman and naturalist; the latter, however, in a somewhat homœopathic proportion. He is also a teleologist. Thus he tells us that—"Most animals' powers of reproduction are so great, *evidently with a view of directly or indirectly affording food to man*, that but for some such check* before the appearance of man upon the scene they would have crowded each other out, and have died miserably of starvation." Without any further examination of the logic of this passage we will call attention to the words we have italicised. Amazing fecundity is certainly no especial attribute of species which contribute to the support of man. It is possessed to a fearful degree by the rat and the mouse, the locust and mole-cricket, the crane-fly, the ant, the aphis, the turnip-fly, the phylloxera, and the Colorado potato-beetle. Were anyone disposed to take a pessimist view of creation, and give a theory of the rapid increase of animals exactly opposite to that of Mr. Morant, he could not be easily refuted. Like Waterton, our author is fully convinced of the injury done to our fields and gardens by the ravages of insects. In support of this view—which, indeed, no competent authority will for a moment feel inclined to question—he appeals to the evidence given before a certain "Select Committee appointed in 1873 to inquire into the advisability of extending the protection of a close season to certain wild birds not included in the Wild Birds'

* *I.e.*, the existence of carnivorous creatures.

Protection Act of 1872." Before this Select Committee appeared Mr. Groome Napier, and gave, as quoted by Mr. Morant, the following most extraordinary evidence :—" The beetles (Coleoptera) are immensely numerous as regards species. They live from three to four years in the larvæ state. The first year they do not do a great deal of damage. The second year they attack the roots of all plants within their reach ; they often ruin the crops of corn, lucerne, &c., on which man depends for food." We can only hope that some qualifying clause has been overlooked by Mr. Morant. Surely both he and Mr. Groome Napier ought to know that, numerous as are the beetles in species, multitudes of them, such as the common ground-beetles (Cicindelidæ, Carabidæ, &c.) and the water-beetles, are, in all stages of their existence, purely carnivorous. The larvæ of others, such as the dung-beetles (Geotrupidæ and their allies), feed upon the dung of animals buried in the ground by the parent insect as a supply for its offspring. Others, again, both in their preparatory stage and when mature, nourish themselves upon putrid animal matter. The larvæ of the long-horns and Buprestids feed on timber, and the weevils—noxious as many of them are—in the buds and fruits of trees. In short, it may be safely maintained that not one-tenth of the species of Coleoptera destroy the roots of plants ; and Mr. Groome Napier, if he has really used the language here quoted, without any saving clause, grievously slanders a multitude of beings, many of which are the most faithful allies of the farmer and gardener.* We think that Charles Waterton would neither have written such a passage nor quoted it, excepting with the intention of inflicting a severe castigation upon its author. But whilst Mr. Morant agrees with the worthy Squire of Walton Hall as to the importance of small birds in rurul economy, he dissents from him altogether as to the causes of their decrease and as to the means to be taken for their preservation. Waterton considers that the worst enemy of the feathered tribes is man, and especially that variety of man known in modern England as gamekeepers. These he pronounces to be " privileged scourges of animated nature," " unrelenting butchers of our finest and rarest British birds." Mr. Morant,

* Not being quite sure at the moment whether Mr. Groome Napier was to be regarded as a naturalist or as a sportsman, we turned to our General Index, and found that he is the author of a work entitled the " Book of Nature and of Man," from which we extract the following passage :—" They (lichens) have a strong resemblance to cancers (*Morbus Brightii*), if they are not to be classed together." Cancer a synonym for *Morbus Brightii* !

on the contrary, maintains that—"It is a great mistake to suppose that man drives away birds by cultivating the surface of the earth. He feeds hundreds in feeding himself, and they are infinitely more numerous in our gardens and on our farms than in the primeval forest where his foot never penetrates, or on the great fertile plains where he has not yet turned the earth." To this passage we shall have occasion to return. He seems indignant at the statement of naturalists that "gamekeepers are ignorant and cruel, and do more harm than good." He asserts that "the sorts of birds they (gamekeepers) kill can be counted on their fingers and their numbers in scores, whilst the sorts of birds they protect are counted by hundreds and their numbers by tens of thousands." The case mentioned by Mr. Stevenson before the Committee, of a Norfolk gamekeeper who said that he shot the nightingales and took their eggs lest they should disturb his pheasants in the night, Mr. Morant disposes of by the clever surmise that the keeper might be amusing himself at the expense of his listener!

Whom, then, ought we to believe? Mr. Morant and the "game-preservers," or Waterton and the "bird-preservers?" Now though, as we have already mentioned, we have little faith in Waterton in matters of inference or of generalisation, in a question of direct observation we doubt if he has ever been found mistaken. He was himself a country gentleman and a sportsman, and certainly would not have denounced the gamekeepers as he did without sufficient reason. Our own observations fully confirm what he has advanced. Many a time have we grieved to see not merely hawks, magpies, crows, and jays, but owls, woodpeckers, goatsuckers, and birds of many other kinds, nailed up against the gable end of a keeper's lodge. Of our own knowledge we endorse the remark of one of the gentlemen examined before the Committee, that "an average gamekeeper kills everything as vermin except what is in the game list." They suspect all animated nature of harbouring designs against their pheasants or "birds," and where there is the shadow of a doubt they act as if there was convictive evidence. An enlightened employer may sometimes attempt to restrain their mischievous zeal, but behind his back the havoc will go on. We have lately read an instance of a keeper who, when rebuked by his master for having shot a goatsucker, and told that the bird was perfectly harmless, replied "Well, sir, but it's a narsty flopping thing."* As

* *See* Science Gossip, iii., 17, and v., 179.

to ignorance, Mr. Morant himself quotes, without any expression of scepticism, the story "of a gamekeeper who told the Rev. Mr. Tristram that 'he was sure the cuckoos turned into hawks in the winter, for if not, what became of them?'" We have ourselves heard German gamekeepers —who are, to say the least, quite as intelligent and well-informed as their English brethren—narrate the most frightful and grotesque stories concerning vipers, salamanders, and newts. But we shall easily understand what must be the behaviour of the gamekeepers towards birds and beasts by a reference to their conduct to men. As they classify all the lower animals under two heads, game and "varmint," so they view all mankind either as game-preservers or as poachers. A botanist or entomologist, however strictly he may abstain from any act of trespass, is always in danger of annoyance and insult in districts where keepers are rampant; and, in a manner perfectly analogous, whatsoever bird or beast is to them strange or novel, that they destroy.* But how are we to get over the evidence of Mr. Morant, who flatly asserts the contrary, and who, mistaken as we hold him to be, is evidently sincere? Let us examine, in the first place, his point of view. His method of re-adjusting the disturbed balance of Nature we have already pronounced strange. He tells us, substantially, that if we will only preserve game by a diligent application of the system now in vogue, and especially by shooting down all hawks, jays, crows, and ravens, the end will be gained. The small birds, whose services we require to rid the land from insect pests, will be benefitted quite as much as the game. If Mr. Morant is in the right the game-preserver must be regarded as a public benefactor. The plea, it must be admitted, is highly ingenious, but we doubt if it can be pronounced valid. He sometimes forgets his zeal for the small birds. Thus, speaking of the black grouse, he exclaims—"Not a naturalist has a word to say for him, while the disappearance of such birds as the siskin or garden warbler is constantly regretted." Naturalists might reply that—leaving game birds to the protection of their numerous and influential *friends*, the game-preservers—they plead for those that have hitherto had no protector. At times, too, if not actually inconsistent, he acts very decidedly up to the

* To prevent any misunderstanding, we may state that we have not the least sympathy for the poacher. He very frequently graduates higher in the school of crime, and becomes a burglar, a sheep-stealer, perhaps a garotter: at the best he is but a gamekeeper *in opposition*. The two are respectively convertible, just as are the conspirator and the *mouchard*, or the martyr and the persecutor.

maxim that circumstances alter cases. Thus in most parts of Britain he would extirpate eagles, hawks, and ravens, in the most thoroughgoing style. He describes, in a manner at once graphic and pathetic, the "sufferings of the young grouse when their mothers are taken,"—the "screams and helpless terror of the old birds, and the feeble efforts of the little ones to escape." But in a "deer-forest"—say rather in a "deer-desert"—he would protect golden eagles to keep down the hares, and falcons to kill the grouse! Yet he questions the sincerity, or at least the consistency, of those who "pretend to care for birds, and yet harbour and encourage cats." Were Waterton's system of tolerating the carrion crow to become general, he maintains that it would be impossible to rear chickens or ducklings except under a net. Yet he recommends the preservation of the fox, and counsels farmers to guard their poultry against his depredations by means of wire net-work. In mountainous regions, however, the fox is to be shot, trapped, or poisoned, as most convenient. Rabbits are to be "ferretted down" till there are only enough left to serve as food for the foxes. He thinks that eggs require fresh air and warmth, and that there is "*nothing more unpleasant than the smell of wild flowers round a pheasant's nest.*" So the very flowers, we presume, are to be extirpated! It is hard to trace, in these recommendations, either humanity, or a desire to promote the interests of agriculture, or the wish to protect useful and beautiful beings, or to establish a new and improved "balance of Nature," or anything but the love of "sport."

Then the small insectivorous birds, whom we desire to protect, have other enemies beside those enumerated by Mr. Morant. The shrikes make sad havoc among them at times; but these minor birds of prey are utterly unable to overpower a partridge, a pheasant, or a grouse, and—shall we say *therefore*—Mr. Morant passes them by undenounced. There is very considerable suspicion, if not positive proof, that the squirrel at times makes free with the eggs and the young of small birds, but he, too, escapes our author's censure.*

We have further to ask why, if—as Mr. Morant seems to hold—hawks and crows are the great obstacle to the increase of beautiful and useful birds, these latter are found to decrease, *pari passu*, with their supposed destroyers? Yet this is undoubtedly the fact. England is decidedly growing poorer in species of birds. Kinds once common are becoming rare; those formerly rare are in many parts verging

* The squirrel undoubtedly destroys pears, peaches, and plums, and even the blossoms of the cherry.

upon extermination. But is it not almost preposterous to ascribe this change to the depredations of species which are themselves on the wane? Should we attribute, *e.g.*, a decrease of game in any district to poachers if these worthies were all the while becoming scarcer and scarcer?

The causes to which we should ascribe the decrease of ornamental and useful birds are many, but they may all be traced to man rather than to hawks, eagles, or crows. Foremost comes, as we have already maintained, the gamekeeper. Mr. Morant, in seeking to deny that this "assassin in velveteen" shoots down anything but predatory birds, reminds us of Waterton when maintaining that serpents never act on the aggressive. Each of them is trying to prove a negative which one affirmative instance to the contrary must at once overturn. Mr. Morant, when surveying Nature, seems to have a pheasant's egg before one eye and that of a partridge before the other, and views all phenomena through this very perplexing medium.

We might even ask, with Waterton, whether the very game birds themselves do not suffer quite as much from their official protectors as from their natural enemies? In his Essay on the Carrion Crow, Waterton remarks—"This man probably never reflects that in his rambles to find the nests of the birds he has made a track which will often be followed up by the cat, the fox, and the weasel, to the direful cost of the sitting birds; and moreover, that by his own obtrusive and unexpected presence in a place which ought to be free from every kind of inspection, whether of man or beast, he has driven the bird precipitately from her nest, by which means the eggs remain uncovered. Now the carrion crow, sweeping up and down in quest of food, takes advantage of this enforced absence of the bird from her uncovered eggs, and pounces upon them. Had there been no officious prying on the part of the keeper, it is very probable that the game would have hatched its brood in safety, even in the immediate vicinity of the carrion crow's nest,—for instinct never fails to teach the sitting bird what to do. Thus in the wild state, when wearied nature calls for relaxation, the pheasant first covers her eggs, and then takes wing directly without running from the nest. I once witnessed this, and concluded that it was a general thing. From my sitting-room in the attic storey of the house I saw a pheasant fly from her nest in the grass, and on her return she kept on wing till she dropped down upon it. By this instinctive precaution of rising immediately from the nest on the bird's departure, and dropping on it when returning, there is

neither scent produced nor track made in the immediate neighbourhood, by which an enemy might have a clue to find it out and rob it of its treasure."

"Keepers may boast of their prowess in setting traps (and in testimony of their success they may nail up the dead bodies of carrion crows against the kennel wall); but I am of opinion that if the Squire could ever get to know the real number of pheasants and hares which have been killed or mutilated in those traps, he would soon perceive that he had been duped by the gamekeeper. The frequent discharge, too, of the keeper's gun, though it may now and then kill or wound a carrion crow, still will infallibly drive away the game in the end, and oblige it to seek some more favoured and sequestered spot." This is cogent reasoning, and we do not see that it is anywhere fully met by Mr. Morant. The disturbing effect of the discharge of guns he himself admits. He tells us that in very remote parts of Scotland he found the grouse "as tame as the boobies which the sailors knock on the head in the South Sea Islands." They actually declined to fly up and be shot in the only orthodox manner, and required to be gradually educated into shyness.

Secondly, as a cause of the increasing scarcity of many interesting birds, we accuse not the sportsman,* in the ordinary English sense of the word, but the troops of roughs who on public holidays sally forth from our towns into the surrounding country and fire promiscuously at everything having wings and feathers, no matter on whose property. If we really wish to protect birds we must put severe restrictions upon this class; they are worse than either gamekeepers or poachers.

Thirdly, we must blame the bird-catchers. Scarcely a native bird having any beauty in its plumage or any sweetness in its song can escape the attentions of these marauders; it is trapped, carried away a prisoner, and generally dies at no very distant date, from want of care and from improper food. The wild birds are indeed in the enjoyment of a nominal "close time." Bird-catchers are prohibited from plying their vocation during the breeding season, and nest-robbing is of course made altogether illegal. But the protection is merely nominal; it is not the duty of any person to see that the Act is duly enforced. The

* We should have nothing against the sportsman if he would only do two things:—first, eschew anti-vivisectionism; and, secondly, make better use of his splendid opportunities for the study of animated nature. Too often, however, he regards birds as mere moving targets.

inhabitants of the outskirts of London and of the suburban villages may see, every Sunday morning during the so-called "close season," troops of bird-catchers going forth to their vocation, and returning about 10 to 12 A.M., the cage holding their prisoners being covered over with cloth, so that no one may be compelled to see how the law is being broken. As for the police, they are too busy watching the movements of the *bona fide* traveller ever to ask an inconvenient question as to the contents of those mysterious packages slung over the shoulders of the unwashed depredators. Would it not be possible to form an association for the enforcement of the law?

Lastly, we must point out, as a cause of the decrease of our beautiful and useful birds, the destruction of the woods, and the removal of the old hedges and hedgerow trees, which once redeemed English rural scenery from the "mad monotony" of the cultivated lands in most parts of the Continent. Here we have again to differ from Mr. Morant, who thinks it a mistake to imagine that man drives away birds by clearing and cultivating the earth. He thus falls into that most dangerous kind of error—a half truth.

In the depths of the primæval forests neither birds nor insects are very numerous, as Messrs. Bates and Wallace take occasion to point out, and as we have in former days learned to our cost. It is along the margin of a clearing, at the edge of the lake, the river, or the savanna, or where the woodland meets the cultivated land, that both species and individuals are most numerous. But if you clear the forest entirely away, and convert the cultivated land into a treeless, bushless waste, divided merely by wire fences, or by low thin lines of stubbed thorn hedges, incapable of sheltering the smallest bird, then you get rid of most insects, save those that prey upon your crops, and of the birds that would have kept the latter in due check. This denudation of the country has been tried in Spain, and the consequence is that the land is desolated with grasshoppers, and that forests are being now planted at national expense in order to afford shelter to insectivorous birds. The system of enormous fields divided merely by rail or wire fences has been largely adopted in the Western States of America, and from those States come heavy and well-founded complaints of the ravages of grasshoppers and vermin in general. Small birds cannot be expected to fly from 3 to 4 miles for every caterpillar or worm they carry home to their nestlings. If we wish to preserve them, and to secure the benefit of their services, we must see to it that there is suitable cover

in abundance, and within a reasonable distance of the lands they are expected to clear of insect depredators; otherwise —were every hawk, raven, or other carnivorous bird hunted down and done away with—our finches and warblers would still be few. We will even venture the opinion that were our hedges and hedgerow trees restored to their old condition, and were human depredators put under wholesome restraint, we might have a better supply of small birds than at present, even if hawks, magpies, and jays were less severely dealt with.

We will now take a brief glance at the birds and beasts of prey whom Mr. Morant "tries for their lives," and, with one exception, condemns. He shows great zeal in overruling any plea that may be urged in favour of the accused, and goes on the principle that no amount of benefit conferred upon mankind can atone for any offence against game. Though neither hostile to game-preservers nor desirous of the extirpation of pheasants, we shall not admit this doctrine.

The golden eagle we cannot defend: he certainly carries off lambs to a serious extent, and may attack children. Against the sea-eagle nothing is proved.* The buzzard, which follows next on Mr. Morant's list, may certainly now and then secure a grouse, but he is too slow on the wing to be a successful bird-hunter. On the Continent he is known as a devourer of mice, rats, and snakes, which latter he attacks with much skill and judgment. The hen harrier is next examined, and condemned as a great destroyer of grouse. It is admitted, indeed, that the crop of an old male, when shot, was found to be full of wireworms. Still we fear that the balance of evidence is against him. There are other destroyers of wireworms, quite as efficient and more trustworthy.

The falcon is unquestionably a bird-devourer who rarely partakes of any other prey; nor do we know of any plea that can be urged in his favour. Little, if any, better is the case of the sparrowhawk, unless he occasionally varies his diet with a mouse. Further and closer observation would therefore here be desirable. The kestrel, or windhover, receives a half-grudging acquittal:—"When mice are plentiful," says Mr. Morant, "he seldom takes birds; but he will not starve, and he well knows that the little newly-

* By the way how many sheep does the "useful dog"—as Mr. Morant calls him—destroy in a year? In the State of Georgia some 28,000 per annum! We may thank the dog-tax that the damage in Britain, though very serious, does not reach so appalling an amount.

hatched pheasants and partridges are not bad eating as a change." The gamekeepers, who have "nearly all at some time or another shot him in the act" of bird-killing, have not impossibly confounded him with the sparrowhawk. Waterton, who had many kestrels in his park, observed that the small birds never seemed alarmed at the approach of the kestrel, whilst there was great and general consternation if a sparrowhawk came in sight. Nor did an examination of their nests reveal feathers or other proofs that birds had been brought for the food of the young. Still Waterton admits, on the evidence of his friend Mr. Bury, that the kestrel "will occasionally make a meal on the smaller birds." Granting this fact, we yet maintain that as one of the best destroyers of mice his services far outweigh his demerits, and sound policy demands his preservation.

The owl, too, receives a like kind of doubtful acquittal, and is at any rate pronounced "the very best mouse-destroyer we have,"—which is certainly true.

In the raven we can see no redeeming features. Not merely does he swallow young birds whole, without enquiring whether they are on the game list or not, but he attacks young lambs and pecks out their eyes, and, as he performs no services which can in the smallest degree compensate for this mischief, he cannot be allowed to exist in a cultivated country. The carrion crow, the Royston crow, the magpie, and the jay have a better account to render: they destroy millions of noxious insects, and if slightly injurious during the hatching season they are eminently serviceable during the rest of the year. The magpie visits the backs of sheep and oxen, and makes a careful search for vermin. The jay is decidedly the least carnivorous of the group, a large part of his diet consisting of peas, beans, and fruit in summer and autumn, and of acorns in winter. These depredations often expose him to death at the hands of gardeners and farmers, but they form no part of Mr. Morant's charges. We should be half inclined to say—"Defend your gardens against the jay by netting, and let him live." Concerning the Royston crow Mr. Morant says—"We must own we once opened the crops of some fully-fledged young hoodies, and found them full of insects, principally beetles. But then their ancestors had eaten eggs for so many years in that country that there were no birds left to lay any within three miles of their nest." We wonder on what evidence this assertion is founded.

The polecat, the stoat, and the weasel come next under examination, and we fear we must approve the sentence

passed upon them by Mr. Morant. These little animals are most determined and bloodthirsty destroyers, not merely of game, but of poultry and of small birds. Waterton says that—"As wrens and robins and hedgesparrows hop from spray to spray, just a few inches from the ground, it seizes them there." He continues—"I once saw a weasel run up an ash-tree, and enter into a hole about 10 feet from the ground. A poor starling had made her nest in it, and as she stood wailing on the branch close by, the invader came out with a half-fledged young one in his mouth, and carried it off." Still Waterton and we believe several naturalists of the present day advocate the preservation of the weasel tribe, on account of the havoc they make among field-mice and rats. "That man only," says Waterton, "who has seen a weasel go into a corn-stack can form a just idea of the horror which its approach causes to the Hanoverians (*i.e.*, rats) collected there for safety and plunder." He winds up his Essay on the weasel by remarking that—"In it may be found the most efficacious barrier that we can oppose to the encroachments and increase of that insatiate and destructive animal, the stranger rat from Hanover." But the remedy, if remedy it be, is very little better than the disease. How is it, further, that the weasel, which a century and a half ago was undoubtedly more numerous than it is in our days, and was less interfered with, still allowed this strange grey rat to become such a formidable interest in the country?

Let us now examine the case of the hedgehog. This unfortunate animal is also tried for his life and convicted. "The hedgehog is the last wild beast on our list,—the 'hypocritical hedgehog,' as Mr. Knox calls him, and 'the most insatiable of all ovivorous British quadrupeds,' whatever his well-meaning and amiable friends may say to the contrary."

"In innumerable instances this little beast has been detected whilst destroying eggs and young birds. Asleep all day, never seen by man unless a dog hunts him out of a hedgerow, he is busy and active enough all night. Can anyone doubt that he is continually finding nests, or do they believe that he ever passes an egg without eating it?

"He has probably no enemy but man, and if man did not reduce his numbers he would do incalculable mischief. Our friends the birds will catch all the insects he is supposed (!) to devour, and we will most certainly do without him as far as possible, hoping to see them [*qy.* the insects?] much more numerous in his place."

In reviewing and reversing this unjust judgment we cannot

help expressing our surprise that Mr. Morant should not have taken the trouble to make himself a little better acquainted with the habits of the unfortunate animal he so sweepingly condemns. We admit that Hoggie is semi-carnivorous, and preys to some extent upon eggs and young birds—when he can catch them. We have known him devour chickens. We have it on good authority that a tame hedgehog once seized a kitten, which was with difficulty rescued by its exasperated mother. We fear that the stories of his occasional raids upon strawberries and windfall apples and pears have some foundation in fact, although no affirmative instance ever came under our own observation. But in spite of all such transgressions the scale must turn in his favour. He is not "supposed," but proved, to devour insects, as was recorded by old Gilbert White, who found fragments of the elytra of beetles among his excreta. His propensity for destroying cockroaches and crickets is known even to the veriest Cockney whose acquaintance with the British fauna was ever gleaned among the bird-fanciers of Whitechapel or of the Seven Dials. The hedgehog's share of work can never be satisfactorily performed by the birds, however they may be multiplied. He gobbles up slugs and snails in the night, their chief time for doing mischief, when the blackbird and thrush are fast asleep, with their heads tucked under their wings, and dreaming perhaps of ripe cherries; he hunts for insects in the bottoms of hedges, and even under ground, where birds are little likely to take up the chase. Hence he is now officially recognised by the French Government as an "agricultural labourer" whose preservation is formally and urgently recommended. But he has still higher claims. Long ago he has been known to be the chief destroyer of the viper, playing in Europe a part similar to that of the secretary-hawk in Africa, and of the mungus in India. Dr. Lenz, a most able and accurate German observer, who studied natural history from a practical point of view, placed the snake-eating habits of the hedgehog beyond all doubt. We have repeatedly witnessed Hoggie tackling a viper, and can testify that the bites of the snake, though they occasionally took effect upon the snout of his enemy, had no more influence than the prick of a needle. The result of the combat was never doubtful, the viper being invariably crunched up with an evident relish. We have known a belt of forest, where we had bagged many a viper, to be cleared of these reptiles by a pair of hedgehogs who had taken up their quarters and reared their brood there. According to a paragraph in "Land and Water,"—

"Vipers abound in the Gironde, and the hedgehog is the declared enemy of the reptile. Since so many hedgehogs have been destroyed vipers have increased at a fearful rate." We are inclined to think that the same cause may have led to the recent multiplication of vipers in England. Now, to form a correct estimate of the services of the hedgehog in keeping down this reptile, we must remember that the bite of the viper is not an insignificant affair. We have known fatal cases in Central and South-Eastern Europe. A young man died last summer from the bite of a viper, received on Leith Hill, Surrey, although medical aid was speedily procured. A French physician, who has published an account of a large number of cases, finds that 20 per cent of the persons bitten succumb to the effects of the venom. Surely, then, a creature which rids us of an evil so serious might claim a better doom than extermination, however many eggs it might devour. We had rather be deprived of pheasants and partridges than be overrun with vipers.

As regards the rat, no naturalist pleads for him. Waterton and Mr. Morant are here perfectly agreed, although they differ as to the means to be employed for his destruction, and although the chapter which the latter allots to the rat is chiefly taken up with a denunciation of "his supposed antidote, the cat." One fact is, however, certain; both rats and mice have of late years enormously increased, and have in some countries become a perfect plague. In Liddesdale the field-mice have devoured even the very roots of the grass. The fields are literally riddled with their holes, and the farmers are in despair. Is it not therefore possible that this increase is due to the destruction, or at least the decrease, of their natural enemies, without any general and organised increase of artificial remedies against their encroachments? The natural enemies of the rat and mouse are the terrier, cat, ferret, polecat, stoat, and weasel; among birds, the larger hawks and the owls; and among British reptiles, the viper. The first of these, the terrier, is, if we are not misinformed, now no longer to be openly and safely used for the destruction of rats, thanks to our humanitarians, who always select some objectionable object for their sympathies—one day rats, another day garotters. It is certainly hard if a farmer, on removing his stack or clearing out his barns, may not send Mustard and Pepper in to seize the grey marauders. The cat is decidedly under-rated by Mr. Morant. We certainly love her not, but so long as mice and rats are common so long Pussy will remain a necessary evil. The increase of field-mice has been most marked where

cats are systematically trapped by game-preservers. Says Mr. Morant—"Does he ever ask himself what these cats go to the woods for? Certainly not to catch rats and mice, which are far more numerous close at home." We doubt all this: leaving field-mice out of the question, we have known many cases where houses much infested with mice were comparatively clear during the harvest season. Mr. Morant himself speaks of the rats venturing out into the hedgerows in the summer months. We maintain that when Pussy lurks among the ripening grain, and springs upon either mouse or rat, or bird engaged in plunder, she is doing mankind good service. The ferret is, of course, a useful agent in the hands of the professional rat-catcher. As for the other animals of the weasel tribe, we have already expressed our mistrust of them. Among birds the great horned owl is a splendid rat-hunter, though we fear Mr. Morant would not approve of his occasional forays upon game. The viper will kill both mice and rats, but we cannot recommend him for toleration even on that account. There are few better rat-hunters in the world than the death-snakes of all countries. Singularly enough the mice and rats, under certain circumstances, retaliate in kind: they eat the eggs of serpents; they attack the young brood, and the adults also when torpid from the winter's cold. The best methods of dealing with rats are already known, and merely require putting in force. The abolition of scamped work about the foundations of houses, the plentiful use of gas-tar or asphaltic preparations in such places, the construction of ovoid drains and sewers where the vermin may find no clinging-room, glass or stoneware pillars for the support of corn-stacks, will greatly limit its sphere of mischief. Poison for rats may best be made up in the shape of candles. The marauders drag these into their holes without suspicion, whilst there is no fear of their being inadvertently eaten by children, or even, we think, by dogs.

So much for the game-destroyers, actual or suspected. Mr. Morant has not a word to say against that loveliest of our native British birds, the kingfisher, nor against the waders and the water-fowl. He praises Waterton as having been in advance of his age in the protection he afforded to these interesting but persecuted creatures, and recommends the owners of parks and manorial domains to follow his example. But what will the anglers say? May they not take exception to a fish-catching bird on the same grounds, and with as much right, as the sportsman brings forward in his protest against hawks and ravens? The diet of the

kingfisher is known beyond the shadow of a doubt. The waders watch on the bank of a pond or in the shallows of a stream till they find opportunity to spear some passing fish: they prey, indeed, also on lizards, frogs, snakes, worms, and insects. But if the angler takes the same view of the case as does Mr. Morant with respect to the carrion crow or the magpie, he will demand their condemnation. Even certain water-fowl, such as the swan, are accused of devouring the spawn of fishes deposited at the bottom of rivers and ponds. We thus find ourselves on the threshold of a serious difficulty. Three conflicting claims are urged. Mr. Morant and the game-preservers bid us destroy the birds of prey and the crow tribe, and preserve all other winged creatures. The anglers, on the contrary, demand the extirpation of the waders and the kingfisher, whatever is done with the Raptores and the small birds. Lastly, but certainly not least, the farmer and gardener pronounce sentence of death on all devourers of seeds and fruits. Here, then, is a "very pretty quarrel." None of the contending powers is willing to abate its own pretensions, though each recommends concession to the other two. The naturalist, if appealed to, stands aghast, and doubts whether he can save any of the feathered race. We are thus led up to a further question, upon which an absolute decision would be premature. We have so far provisionally assumed that the small birds are our benefactors in an unqualified sense, and that their unlimited increase would be desirable; but this is not proven. Few of them are purely insectivorous, as every gardener knows; but, on the other hand, many of the seed-eaters are highly useful by limiting the propagation of weeds. Thus the goldfinch, now becoming rare in many parts of England, is particularly fond of thistle-seed. It is generally said that if we encourage the birds we may avail ourselves of their services, and yet prevent them from doing mischief by the use of netting. We may, indeed, protect wall and espalier fruit in this manner, but to net over entire orchards and fields is impracticable; besides, nets—if they prevent fruit-stealing—will likewise interfere with insect-catching. Further, the alleged depredations of some birds are not deferred until the crops are ripe. The bullfinch, chaffinch, and titmouse are charged with pulling off the buds of fruit-trees. A sentinel with a gun will assuredly scare away the innocent birds as well as those really guilty. One small bird Mr. Morant declines to protect. The sparrow may almost be called a winged rat, from the extent and variety of his depredations: he catches insects during the

breeding season only, and all the rest of the year he is continually in mischief. The evils which have in some instances seemed to follow his local extirpation are probably due, as Mr. Morant very judiciously suggests, to the simultaneous destruction of other birds. Perhaps his worst attribute is that he drives away birds which are at once more useful and more beautiful than himself. His attacks upon the nests of the swallow are well known, and he sometimes drags the helpless nestlings from their cradle and throws them to the ground. One of the most difficult points in practical ornithology is how to preserve other birds without encouraging the sparrow also. Mr. Morant remarks—"Netting them in winter is quite a legitimate way of destroying them, and if they are shot from a trap next morning they will have a chance to escape." We quote this passage as showing a curious idiosyncracy of the mind of the sportsman. Either the sparrow deserves to die or he does not. If he deserves death, why give him a "chance" of life? If he does not deserve it, why seek to kill him? We hold that whatever is worth doing at all is worth doing with absolute certainty, and that "chance" wherever possible should be altogether eliminated.

We see, in fine, that though a "balance of Nature" exists it is incompatible with the presence of civilised man, who is unable to avoid disturbing it even if anxious for its preservation. Hence, how much soever we may approve of the practice of Waterton and of his disciples, in a number of cases we cannot accept them as safe guides. Their general principle, indeed, as Mr. Morant argues, is fundamentally vitiated by the fact that Waterton carried on his observations in a country where all the more formidable beasts of prey have been long ago extirpated. If it be wrong now to root out the polecat, why was it right in former centuries to exterminate the wolf, the bear, and the lynx? To reduce *ad absurdum* the system of letting animated nature alone, we need only try its operations in Assam, Java, the Cape, or any other country still blessed with man-devouring cats and with thanatophidia. Regarding therefore this original balance as provisional, and suitable merely to the absence of man, we have to establish a new equilibrium. But here we have quite as decidedly to reject Mr. Morant's fundamental principle, which is evidently no safe guide. Still more strongly must we protest against the conduct of those who, without enquiry, kill every creature they suspect of mischief.

But having thus expressed our dissent from the views of

others, we must expect to be asked what method we recommend for finding and establishing a new and satisfactory balance? To this we cannot reply in a neat prescription of some half-dozen lines. Much of the knowledge necessary for a decided and final answer has yet to be obtained. We can merely recommend that every case be for the present decided upon its own merits; that the habits, and especially the diet, of all creatures be much more carefully studied than has yet been attempted; and that sentence of extirpation be not passed till the injury done by the species concerned has been proved to be greater than the benefits it confers. But where such proof has been furnished, then let there be no trifling. "*Frappez vite et frappez fort.*" Let there be no "chance" given to the offending animal; let no young ones be spared that some one may exercise his courage and his skill by shooting them down when mature! A few general considerations may assist. There must be, of course, no mercy shown to parasites, internal or external. To say that these creatures "are designed to make people clean" is, as a contemporary remarks, "simply absurd." The cleaner people are the more fiercely they are attacked by bugs and fleas. We once passed the night in a *salasche* high up in the Carpathians, and were nearly devoured, whilst the filthy natives slept on unmolested. War must next be waged against all large Carnivora, and against all or most small Herbivora and Omnivora. It will be useful to take into consideration each separate duty or function which we require animals to perform; to examine what species executes it in the most effectual manner and with fewest drawbacks, and to give that species the preference. Thus the fox, the mole, the hedgehog, the weasel, and the crow tribe, will all destroy cockchafers. But the fox and the weasel—and, in Mr. Morant's opinion, the crows also—have so many vices that man may refuse to employ them as cockchafer-devourers, and may hand over the work to the mole and the hedgehog. Again, the hen harrier and the pheasant both prey upon wireworms; still the pheasant is the safer workman in this department, and we should accordingly give him the preference.

But the greatest difficulty lies in the fact that our animal allies do not spare each other. Thus several beasts and birds have been praised as beetle-destroyers. But among beetles there are multitudes inoffensive; multitudes—as we have already shown—positively useful to man. But the weasel, the crow, and the mole devour the burying-beetle, the dung-beetle, the jardinier (*Carabus auratus*), just as

greedily as the cockchafer and the mole-cricket. The weasel will assassinate his fellow insect-hunter, the mole, just as readily as the rat or the field-mouse. Many naturalists would pronounce the stork useful; but no small part of his time and attention are devoted to catching and swallowing the frog—one of our best friends. The hedgehog kills vipers and noxious insects, and also beneficial ones. The small birds capture the destructive cabbage butterfly, and pick its caterpillars from our fields and gardens, and in so doing they merit well; but they quite as eagerly destroy the "red admiral," the "painted lady," and the "peacock," which help to keep down noxious weeds. Our feathered friends also, when opportunity offers, make a meal of the dragonfly, who in his adult state is zealous as the swallow in clearing the air of winged vermin, and in his earlier and aquatic condition is no less useful in devouring the larvæ of the gnat. All carnivorous insects which inhabit the water merit especial protection. The ladybird helps to rid our fruit-trees, our hop-gardens, and our rosaries of the loathsome aphis, but whilst engaged in this good work it is devoured by the very birds which we are advised to spare and shelter. The ichneumons and other parasitic insects are praised for keeping down caterpillars; yet here again harmless and useful species are attacked quite as frequently as those which are noxious.

Thus our animal allies, like riotous and ill-disciplined troops, exchange blows with each other when they ought to present a front to the common enemy.

Another point remains: we talk of carnivorous and herbivorous species, but in multitudes of cases the diet of animals is by no means so rigidly defined as is commonly supposed: they have their preferences, but rather than starve they are generally ready to adopt a substitute. Cows in Norway are known to partake of herrings in the winter, and in milder climates they are decidedly fond of mumbling a bone. What creature is more decidedly predatory than a spider? Yet when "sugaring" for moths, at night, we have more than once seen a grim spider sitting at the edge of the mixture and apparently sucking it up. We have also met with a *Carabus*, one of the most carnivorous of beetles, similarly engaged, and profiting by the hint we fed one of the same species, in captivity, upon bits of apple. From time to time we hear of a bloodthirsty freak on the part of some beast or bird generally considered a pure vegetarian. Indeed how many orders, or even families, can we find which do not count among their members

both flesh-eaters and plant-eaters, and which may thus be said to dwell on the debatable line? Hence we must consider it as not impossible that if some insectivorous bird—say the starling—should increase to a very great extent, and should become pressed for subsistence, it might perhaps be tempted to attack fruits and grain.

II. ON UNDERGROUND TEMPERATURE, WITH A DISCUSSION OF THE OBSERVATIONS MADE AT SPERENBERG, NEAR BERLIN.

By O. Fisher, Clk. M.A., F.G.S.

THERE is no fact more firmly established in terrestrial physics than that the temperature of the rocks of the earth's surface increases with increasing depth. Observations upon this subject have been made in all parts of the world, and the same result has been everywhere arrived at. Even in the frozen soil of Yakoutzk, in Siberia, this increase of temperature is found to prevail, although the ground is congealed to the whole depth penetrated.* In all mining operations this gradual increase of temperature becomes a very serious consideration, rendering human labour at great depths a very severe trial to the constitution of the workman. And indeed it is this circumstance, more than any other, which fixes a practical limit to the depth at which mining operations are possible. It is not the raising the minerals from profound depths, for the resources of modern engineering are quite competent to overcome any difficulty on that score; it is not keeping the mines clear of water, for they are less troubled with its influx at great than at moderate depths; but it is the impossibility of furnishing the men with an atmosphere to breathe in below the temperature of the blood. For when the air has to be conveyed to long distances it acquires the temperature of the rocks,

* The increase of temperature at Yakoutzk, although the soil is frozen even beyond the depth reached, proves that this freezing is owing to the present low mean temperature of the locality, and that it is not a residual effect of a former glacial and still colder period; for if that were so the strata would be now warmer above than below, whereas the reverse is in fact the case.

and no means have been yet suggested which could furnish it at the atmospheric temperature unaffected—or but slightly so—by its long journey through the subterranean passages.

This increase of temperature, though universal, is not everywhere the same. The average is about 1 degree Fahr. for between 50 and 60 feet of descent. Such is the result of very numerous observations. Some of these have been made by drilling holes in the rock in deep mines; others by lowering thermometers in Artesian bore-holes. A Committee was appointed by the British Association to report upon the subject, and their reports extend from the year 1869. Much information upon the matter may also be gathered from the Reports of the Parliamentary Commission upon the Coal Supply.

Unquestioned as is the fact, nevertheless the cause of this increase of heat has formed the ground for much speculation. The most obvious explanation of it is offered by the phenomena of hot springs and volcanos. These show us that, in some places at any rate, much higher temperatures exist at great depths than have ever been reached by artificial perforations. But these phenomena might be said to be local, while the general slow increase of temperature we have been describing exists more or less at every place. Are these phenomena directly connected? Are they indirectly connected? Or, are they altogether unconnected? Various answers have been returned to these questions.

Among "practical men" engaged in mining operations an opinion seems to have prevailed that the increase of temperature in deep mines is due to the pressure of the overlying strata or "cover." We may unhesitatingly dismiss this hypothesis. Pressure by itself cannot develop heat. Where motion is destroyed as motion, there it is that it is converted into heat. Thus the motion which is destroyed when a hammer strikes upon an anvil will develop heat that can explode fulminating powder, or heat a nail red-hot. So, also, the bearings of a wheel become heated by friction, which gradually destroys its motion. But though mountains rise on mountains, there will be no heat produced unless motion of some kind is destroyed. In deep coal-mines an effect called "creep" is caused by the enormous pressure upon the sides of a passage, or upon the pillars left to support the roof, causing the floor of the excavation to swell up. In this case there is immense friction between the particles of the rock, were it not for which the passage would become instantaneously closed. It has been remarked that considerable heat sometimes accompanies this creep,

in which, be it observed, we have not pressure alone, but motion destroyed and converted into heat.

Sir Humphry Davy ascribed the volcanic fires to the oxidation of earthy and metallic bases, supposed by him to exist low down in the interior of the earth. Since water is everywhere present in the crust, if it be absorbed by the abstraction of its oxygen when it encounters the bases, and by the blowing off from volcanic vents of the equivalent hydrogen, then its place must be continuously supplied by the percolation downwards of fresh accessions; and thus a generally diffused increase of temperature was supposed to arise, and to manifest itself by its escape towards the surface.

A very fascinating theory, which has met with support among many scientific men, has of late years been promulgated by Mr. Mallet, who considers the heat of volcanic action to be derived from the transformed work of crushing the rocks of the earth's crust, owing to the contraction of its interior through long-sustained cooling. The cause of evolution of the volcanic heat under this theory is of the same kind as that alluded to above, in the case of creeps; while the ubiquitous increase of temperature in descending into the earth is looked upon as chiefly a manifestation of the generally diffused heat of the interior, by the escape of which the contraction in volume is produced. Volcanic action, Mr. Mallet tells us, is caused by this contraction manifesting itself locally and paroxysmally.

In order to decide what theory best accounts for the phenomena, it is obvious that the first step is to make sure of the facts themselves. In other words—What is the law regulating this increase of heat? At what rate does the temperature augment? This might be supposed to be a point easily settled. It might be supposed that nothing would be easier than to insert thermometers into the rock at different depths in a mine, and to read off their indications; or to lower them into a bore-hole for the same purpose. But there are many difficulties to be overcome, and a host of disturbing causes present. The rock, or the face of the coal in a mine, is affected by the temperature of the air. The air is warmed by the presence of men and horses; it is cooled by the ventilation carefully kept up. Hence the surface of the working is not at the true temperature of the rock. The result of careful experiments made by Sir G. Elliot showed that when thermometers were inserted in the coal in a long-wall working, at distances of 3, 6, and 12 feet from the face, no alteration could be

detected in the indications given beyond 3 feet from the face of the coal; and his observations were accordingly taken at 4 feet. It is evident, as will be seen by what will be said hereafter, that the necessary distance must be governed by the difference between the temperature of the ventilating air and of the seam, and that the greater this difference the farther it would be necessary to bore into the coal to obtain an estimate of its true temperature. If it be attempted to ascertain the law of increase by means of an Artesian bore-hole, there are here also disturbing causes present. The action of the tool warms the rock by mechanical means, the friction or pounding action developing considerable heat. Hence while the work is in progress no reliable observations can be taken, except during intervals of suspension, and then it appears—as will be seen further on—that the interval needs to consist of weeks rather than of days. When the work is completed, and the bore stands nearly full of water, it is the temperature of the water which is obtained on lowering a thermometer into it, and we cannot be sure that that coincides with the temperature of the rock. In fact many reasons may be assigned why it should not do so. Springs may enter on one side and flow out at the other; or they may rise from lower levels and flow away at higher. The very act of lowering the thermometer tends to mix up the differently heated layers of water, and to confuse the result.

A single instance will suffice to illustrate the irregularity of increase referred to. At Rose Bridge Colliery the temperatures were taken by drilling a hole a yard deep at the bottom of the shaft during the process of sinking. If the mean temperature of the surface there be taken at 50° F., the mean rate of increase for the whole depth was 1 degree for 55 feet. But at the successive depths of 605, 630, 663, 671, 679, 734, 745, 761, 775, 783, 800, 806, 815 yards respectively the rate appears to have been 1 degree for 70, 25, 49, 24, 110, 66, 32, 48, 51, 36, 54 feet.

Within the last few years a very deep boring has been carried down at Sperenberg, near Berlin. This has reached the extraordinary depth of 4052 Rhenish feet, or 4172 British feet. A most surprising geological fact about this boring is that, with the exception of the first 283 feet, which were carried through gypsum with some anhydrite, the remainder passed entirely through rock salt. This seemed to offer an exceptionally good opportunity for observing temperatures in a homogeneous rock, which might be expected to be comparatively free from the sources of error arising from

variable heat-conducting power in the layers successively penetrated. The observations in the bore were taken with much precaution, under the direction of Herr Eduard Dunker, Inspector of Mines. A *resumé* of his paper upon them will be found in "Nature" (No. 376, for January 11 of the present year) as contained in the "Ninth Report of the British Association Committee on Underground Temperature." In No. 312 of the same publication (1875) there had previously appeared a notice of a contribution, by Prof. Mohr, of Bonn, to the "Neues Jahrbuch für Mineralogie" (1875), in which that gentleman had commented upon the law of increase which he supposed to be deducible from the observations made in this bore-hole. The result at which he arrived was remarkable enough, and if it could not have been explained away would have justified the conclusion which he drew from it, which was that the source of heat within the crust of the earth was situated within the crust itself, for that after a certain depth was reached—which he put at 5170 feet—the increase would be *nil*. However, in the Report of the British Association just published, it is explained how this result had been arrived at, and it is distinctly shown that Prof. Mohr's conclusion was in reality not based upon the original observations themselves, but that it arose from the form of an empirical mathematical formula which Dunker had assumed to express the law; so that the law which brought the increase of heat to *nil* at 5170 feet was of Dunker's making, and not Dame Nature's. In fact it had been implicitly assumed that the increase would come to an end, and all that Mohr did was to find out at what depth it would do so, supposing that assumption true. A better instance can hardly be required to show the extreme caution necessary in accepting the conclusions of philosophers when they are, on the face of them, heterodox. The safe attitude of the mind in such a case is to suspect that there must be some mistake, and the duty of the competent is to try and find it out.

This bore-hole, from the favourable circumstances already referred to, and the care with which the observations were made, deserves full consideration. The temperatures were taken in two manners. In one set of observations the temperature of the water in the bore-hole was observed with a suitable thermometer; in the other an apparatus called a geo-thermometer was lowered, which cut off a portion of water from that above and below it by two disks or bags. A thermometer was enclosed in the space between the disks, and, after the thermometer had been down not less

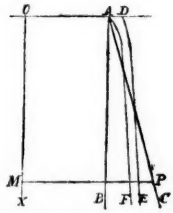
than ten hours, the temperature shown by it was assumed to be that of the rock at the corresponding depth. This was found to differ sometimes by more than a degree Réaumur from the temperature of the water at the same depth, as shown by the first-named thermometer, when the water above and below was not cut off. Now, it can hardly be supposed that so great a difference as this really existed simultaneously between the surface of the rock exposed to the water in the bore-hole and the water itself. On the contrary, it seems evident that the surface of the rock, being exposed to the action of the water, must have assumed the temperature of the water, whatever that might be, at any given depth. But the water was undoubtedly affected by convection currents, and consequently its temperature was not the same as that of the rock *in mass* at a distance from the bore-hole. Hence, when the circulation of these currents was stopped by the disks, the surface of the bore-hole would begin to tend towards the true rock temperature. If the water at any depth was warmer than the rock in mass, its temperature would begin to fall when the currents were cut off, and if the water was cooler than the rock it would begin to rise.

Let us, then, shortly consider the general effect of convective currents as they would affect the water.

These currents arise from the circumstance that when water is warm it expands, and consequently becomes lighter. This expansion is exceedingly small; yet in a substance of such extreme mobility as water it is sufficient to cause the expanded portions to rise, while the denser portions above sink to supply their place. The expanded portions in rising carry their heat up with them, and the cooler portions in sinking reduce the temperature of the lower part of the column. If therefore we had a column of water originally warmed towards its lower portion, but not supplied there with fresh accessions of heat, the whole would, if open to the atmosphere, shortly assume an equable temperature throughout.

Let us now invoke the aid of a diagram to render our ideas more clear; and suppose the depths in the bore-hole to be measured along the vertical line O X, and let lines drawn at right angles to this represent, in proportion to their lengths, the temperatures at the corresponding depths. Let O A represent the mean temperature of the surface of the ground. Then on the supposition that the temperature of the rock in mass increases proportionally to the increase of depth, the temperature of the rock will be represented by a *straight* line, as A C. If, then, the temperature of the

water in the bore-hole correctly gave the temperature of the rock, its temperature would be likewise represented by the same straight line A C. But since it is affected by convective currents this cannot be the case; for they tend to warm the upper portions of the column of water, and to cool the lower. Hence, so far as we have gone, the water in the upper part of the bore-hole will be warmer than the body of the rock, and in the lower part it will be cooler; and in our diagram we must draw a line to represent its temperature, more distant from O X in the upper part, and nearer to it in the lower. It need not, however, be a straight line, the law of increase of temperature being possibly altered. Suppose, then, D E to be this line, or the curve of temperature of the water on the supposition now made. It will be seen that it must intersect the line A C.



There is a further consideration to be taken into account. If the bore-hole is nearly full of water, and of considerable dimensions, it will present a considerable surface to the atmosphere. At Sperenberg the water stood 7 feet from the stage of the bore-pit, and the bore-hole was a foot in diameter. The consequence would be that the open surface of the water would be cooled, sensibly to the temperature of the air. This would bring the curve of temperature of the water at the surface nearly to the same point as the temperature line of the rock there, and it would also have the effect of reducing the temperature of the water throughout the column, below what it would be if it had not this extrinsic cause of cooling. The ultimate result would be that the temperature curve of the water would assume some such form and position as A F. It will be observed that this line also intersects A C; and the signification of this is, that the water in the bore-hole is warmer than the rock mass in its upper portion, and cooler than the rock mass in its lower portion; or, in other words, the water tends to warm the rock in the upper portion, and to cool it in its lower portion. Consequently, if the circulation of the currents be interrupted, the water im-

prisoned between the disks of the geo-thermometer would in the upper portion gradually become cooler, as heat was conducted away from it into the body of the rock; and it would gradually become warmer in the lower portion, as heat was conducted into it from the body of the rock. But at the particular depth at which the two lines A C and A F intersect no alteration would take place.

That such an effect was actually produced is shown by the following table, the first two columns of which are taken from Dunker's Table II., in his paper entitled "Ueber die Benutzung tiefer Bohrlöcher zur Ermittelung der Temperatur des Erdkörpers, und die desshalb in dem Borloche I zu Sperenberg auf Steinsalz angestellten Beobachtungen." The two temperatures marked with asterisks differ from those quoted in the British Association Report from Dunker's quarto paper of 1872 in the "Zeitschrift für Berg-Hütten und Salinen-Wisen." But they are printed as here given in the octavo paper from which they have been copied, and are stated there to be the mean results of several observations. Dunker considered the deepest observation with the geo-thermometer unsatisfactory.

Depth in feet.	Temp. R. Water shut off.	Temp. R. Water not shut off.	Water shut off, consequent alteration of Temp.	Surface Temp. 7·18°. Absolute Increase at each depth in Col. 2.	Being at the rate of 1° R. per No. of feet.	Absolute Increase at depths.	Being at the rate of 1° R. per ft.	Being at the rate of 1° F. per ft.
15	9·40	10·35	−0·95	—	—	—	—	—
30	9·56	10·20	−0·64	—	—	—	—	—
50	9·86	10·40	−0·54	—	—	—	—	—
100	10·16	12·30	−2·14	2·98	33	—	—	—
300	14·60	13·52	+1·08	4·44	45	—	—	—
400	14·80	14·30	+0·50	0·20	500	—	—	—
500	15·16	14·68	+0·48	0·36	277	—	—	—
700	17·06	16·08	+0·98	1·90	105	—	—	—
900	18·50	17·18	+1·32	0·44	455	—	—	—
1100	*19·90	19·08	+0·82	1·40	143	11·72	94	42
1300	21·10	20·38	+0·72	1·20	166	—	—	—
1500	22·80	22·08	+0·72	1·70	118	—	—	—
1700	24·10	22·90	+1·20	1·30	154	—	—	—
1900	25·90	24·80	+1·10	1·80	111	—	—	—
2100	*27·70	26·80	+0·90	1·80	111	7·80	128	57
2300	28·50	28·10	+0·40	0·80	250	—	—	—
2500	29·70	29·50	+0·20	1·20	166	—	—	—
2700	30·50	30·30	+0·20	0·80	250	2·80	214	95
2900	—	31·60	—	—	—	—	—	—
3100	—	32·70	—	—	—	—	—	—
3300	—	33·60	—	—	—	—	—	
3390	36·15	34·10	+2·05	5·65	122	—	—	—
3500	—	34·70	—	—	—	—	—	—
3700	—	35·80	—	—	—	—	—	—
3900	—	36·60	—	—	—	—	—	—
4042	38·25	38·10	+0·15	2·10	310	7·75	212	95

In the above table the first column gives the depth in Prussian feet, at which the observations of temperature were made.

The second column gives the temperatures observed at the respective depths when the convection currents were stopped by the use of the geo-thermometer.

The third column gives the temperature of the water in the bore-hole when the convection currents were allowed to circulate.

The fourth column gives the alteration of temperature at each depth which arose from stopping the convection currents—*minus* when the temperature fell, *plus* when it rose.

The fifth column gives the increase of temperature at each depth over that at the previous one, as taken from the second column.

The sixth column gives the rate of increase at each depth, measured by the number of feet which it would be required to descend to gain an increase of 1 degree Réaumur, on the supposition that the increase per foot remained constant throughout that depth; consequently large numbers show a proportionately slow increase.

The seventh column, like the fifth, shows the increase of temperature at the depth against which the number stands, over that at the depth at which the previous number stands; the object of this column being to obtain averages at longer distances apart.

The eighth column, like the sixth, shows the rate of increase of temperature with longer averages, measured by the number of feet of descent for 1° Réaumur.

The ninth gives the same increase when measured in feet of descent for 1° F.

The difference between a Prussian and an English foot is so small that, for the purpose in hand, they may be considered equal.

In discussing this table the first point to be noticed is that the rate of increase is by no means so equable as, from the homogeneity of rock, it might have been expected to have been. In order to obtain anything like a general law of increase, it is necessary to take the average of the increase at considerable distances apart.

The second point has been already adverted to, viz., that the shutting off of the convection currents caused an increase of temperature in the upper part of the bore-hole, and a diminution of it in the lower. This shows clearly that the temperature of the rock was altered temporarily, by the action of the convection currents, to some distance away laterally from the bore-hole. The hole was lined with three tubings, one behind another, for the upper 440 feet, and, on account of the pre-

sumed convection currents still existing behind the tubing, the observations in the upper part of the column were not considered trustworthy. But this circumstance does not militate against the conclusion at which we have arrived, because whatever effect convective currents might have upon the temperature would be *pro tanto* produced by these concealed currents, so that all they could do would be to lessen the effect of shutting off the currents in the main channel. And we may reasonably suppose that the change of temperature arising from that proceeding would have been still greater than recorded, if the tubing had been in close contact with the rock.

In the discussions of these observations hitherto published* the observations made with the geo-thermometer have been looked upon as giving a near approximation to the temperature of the earth's crust at this locality; and, consequently, it is these which we propose to consider further. Although the sixth column shows that the rate of increase of temperature was far from equable when comparatively short intervals are taken into account, yet, when we pass to the eighth and ninth columns, we observe that on the whole there is a decided diminution of the rate of increase in the lower depths. It was probably this circumstance which induced M. Dunker to assume that empirical formula for the law of increase which led Prof. Mohr to believe that, at the depth of 5170 feet, the increase would be *nil*, and thence to conclude that the source of the heat of the crust must be situated within the crust itself, instead of—as is usually supposed—coming up from the profound depths below. The question which presents itself therefore (and it is a most important question) is—Can this diminution of the rate of increase in the indications of the geo-thermometer be consistent with an equable rate of increase in descending as deep as the observations went into the earth's crust? The answer, that it can, seems to follow from the considerations already made on the effect of convection currents upon the temperature, not of the water only, but of the rock itself. It is obvious that the continued contact of water at a different temperature from that of the rock must alter the temperature of the rock itself where it is in contact with the water. And it has been remarked that, almost beyond dispute, it had actually that effect; consequently the rock in contact with the water must have been cooled in the lower part of the bore-hole. Moreover, the principal cooling

* Since this article was sent in, the writer has met with a course of lectures entitled "Vorträge über Geologie, von F. Henrich, Wiesbaden, 1877," in which the Sperenberg observations are discussed with much acumen.

effect of the currents would occur towards the bottom of the hole, where the cold water coming down from above would tend to accumulate, because there would be no warm currents coming up from below to disturb it.

When, therefore, we enquire how far the observations made with the geo-thermometer can be depended upon as having given the true temperature of the rock in mass, two questions present themselves. First, could the water enclosed between the disks assume eventually the temperature of the rock mass, if the instrument were left down long enough? And, secondly, if it could do so, was it in fact left down long enough?

Before attempting to answer these questions, it may be well to refresh our minds regarding the mode by which a material like rock transmits heat through its substance, and by so doing affects the temperature of whatever may happen to be in contact with it. In such a case the heat is not conveyed from point to point by the fetch-and-carry system which, going on in a liquid like water, is called convection, and has been already described; but each particle of rock receives heat from those in contact with it if they happen to be hotter than itself, and gives up heat to those in contact with it which are cooler than itself, without ever moving from its place. Thus the heat is passed on from particle to particle, much as we see buckets of water passed from one person to another at a fire. This mode of propagation of heat is called conduction, while the power inherent in any substance to transmit heat through itself is called its conductivity; those substances which can transmit heat most rapidly being said to have greater conductivity than those which transmit it more slowly. The conductivity of ordinary rock is small; that of rock salt is said to be comparatively great.

Could, then, the water enclosed by the geo-thermometer recover the temperature of the rock mass if the instrument were left down long enough? It appears that this result could be only partially attained, however long it were left in the bore-hole. The convective currents in the water would affect the temperature of the column at the upper and under surfaces of the enclosing disks of the instrument. And even if the disks were purposely made of such badly conducting material as to prevent any temperature effect from the currents being carried through the disks, still such effect would be conducted round the edges of the disks, through the substance of the rock itself; consequently, however long the instrument might have been allowed to remain down, the temperature of the water enclosed be-

tween its disks would have been to some extent affected by the convective currents; and their tendency in the lower parts of the column would be to reduce the temperature, and diminish the rate of increase.

A method of obviating this source of error is mentioned in the British Association Report, already referred to, as having been lately suggested by Sir W. Thomson. It consists in using a series of india-rubber disks placed at considerable distances apart.

Putting the above-discussed source of error aside, we come to our second enquiry, whether the geo-thermometer was left down long enough for the water between its disks to assume the temperature of the rock mass ?* Let us, then, consider the conditions of the system before the geo-thermometer is introduced. We have a very long vertical column of water enclosed in a cylindrical hole within a mass of rock, the rock extending to an infinite distance both sideways and downwards. The water in the bore-hole at any given depth has, in consequence of the currents, a temperature which differs from that of the rock on the same horizon at a distance from the hole. This temperature of the water may be higher or lower than that of the rock; but we will suppose it lower, as it will be in the deeper parts of the hole. The rock surface of the bore-hole, being constantly laved by the water, has been brought to the same temperature as the water. It necessarily follows from this that if we could examine the temperatures of the rock at greater and greater distances from the bore-hole, we should find them become higher and higher, and we should have to penetrate to some considerable distance before the increase came to an end, and when it did so we should feel assured that at last we had reached rock of the true temperature for the depth. The greater the difference between the temperatures of the water and the rock in mass, the further we should have to penetrate for that purpose, and this would be furthest in the lower portions. Now suppose the currents interrupted by lowering the geo-thermometer, so that the heat ceases to be conveyed away from the rock-surface of the bore-hole; and we will now dismiss the consideration of the effect of the water above and below the disks of the instrument. The heat which

* It appears that at the depth of 1100 feet an observation of ten hours duration gave the same result as one of nineteen hours; whence it was concluded that ten was long enough. But considering the great thermal capacity of water, and the small changes which, except just at first, might be expected in the rock, as well as the difficulty of the operation, this trial can hardly be held conclusive against the probability of a further increase of temperature, if the geo-thermometer had remained down longer.

flows out of the sides of the bore-hole into the imprisoned water is now no longer conveyed away by the currents, and begins to accumulate there. The rock in the neighbourhood of this water begins to get warmer. The region of the true rock-temperature approaches nearer and nearer to the bore-hole, until at last it reaches it, and then, and not until then, the imprisoned water assumes the true temperature of the rock. This process will require time, and that a long time. In the first place, on account of the great capacity of water for heat, it requires much more heat to warm up a volume of water through a given range of temperature than it would require to warm up an equal volume of rock. In the second place, it requires a long time for the heat to travel through the rock to reach the water. And although the conductivity of rock salt may be considerably higher than that of ordinary rock, so that the heat passes more quickly through it, yet there will be a compensating effect in the present instance, because, on account of the greater conductivity, the true rock temperature would not be reached within so small a distance from the bore-hole, so that the heat would have a longer distance to travel before the true temperature could be restored. That the geo-thermometer was not left in place sufficiently long for this purpose seems to be proved by the following instance of the extreme slowness with which the passage of heat under such circumstances takes place. From comparing the British Association Reports for 1872 and 1873 we gather that, in the course of the observations made at the Artesian well of La Chapelle, at S. Denis, it was noticed that the temperature at the bottom of the hole was much affected by the action of the "trepan" or boring tool. It appears to have been expected that this effect would have passed off in a few days. Accordingly, a week after the tool was stopped, observations were taken, and there was found what was thought to be an unaccountably sudden increase of nearly 8° F., in 60 feet of descent, near the bottom of the hole. But in the following year a second set of observations were made "some months" after the boring had been suspended, and the abnormal increase was found to have entirely disappeared. We learn, then, that *a week* was not sufficient for the rock wall of the bore-hole to regain its balance of temperature after having been disturbed through about 7° F. How much longer was required we have not the means of knowing. It seems a necessary inference that a few hours would be quite insufficient for a correct observation, and the time allowed at Sperenberg did not exceed such an interval. The conclusion must therefore follow that, on both the accounts referred

to, the geo-thermometer must have shown in the lower depths a temperature less than the true temperature of the rock of the earth's crust existing at that depth.

The reasons given above seem quite sufficient to account for the diminished rate of increase of temperature shown in the table; and they would lead to the conclusion that in all observations made in bore-holes of a sufficient width to allow convective currents full play, the temperatures taken at the lower parts will—even with the geo-thermometer—be less than the true rock-temperature. The conclusion appears a legitimate one that the diminution in the rate of increase in a bore-hole, even when the convection currents are temporarily shut off, does not necessarily imply that there is any such diminution of the rate in the body of the rock; or, in other words, that the temperature curve within the rock may be a straight line, although that given by the geo-thermometer be concave to the axis of depths. It will also follow that the mean rate of increase for the whole depth within the rock will exceed that shown by the geo-thermometer. The mean increase, for instance, in the above table, between the temperature of the surface and that at 4042 feet, is 1° R. for 129 feet, or 1° F. for 57 feet; and it is stated in the Association Report that, when the observations are corrected for pressure, the mean rate to 3390 feet is 1° F. for 51·5 English feet. This is about the usual average. We may therefore conclude that at Sperenberg the actual rate of increase in the body of the rock is greater than this.

The most important conclusion from the above is that, in spite of the decreasing rate shown at Sperenberg (and also at St. Louis, and perhaps other places), the old assumption may be correct, that for such depths as have been reached the rate—if it could be truly observed, and setting aside local causes of disturbance—is probably a uniform rate, amounting to, or, as the above reasoning would lead us to infer, probably exceeding, 1° F. for 51 feet of descent. Are we, then, to conclude that this rate of increase obtains for all depths, however great? If such be the case, as Sir William Thomson in his late Sectional Address at the British Association (1876) sarcastically observed, "by a simple effort of the geological calculus it has been estimated that 1° per 30 metres gives 1000° per 30,000 metres, and 3333° per 100 kilometres." And since it has been considered that between the two last-named temperatures all—even the most refractory—substances of the earth's crust would melt; therefore it has been concluded that at depths varying from 30 to 100 kilometres (or from about 20 to 60 miles) the temperatures are so great as to melt all known substances.

The argument, we are told, has been used by a geologist that, because the increase has been found proportional to the increase of depth as far as observations have gone, "it is most in accordance with inductive science to admit no great deviation in any part of the earth's solid crust" from the law of proportionality that has been observed as far down as observations have extended. The truth seems to be that the uniformitarian principle had led this geologist (whose name is not mentioned) to argue from the known to the unknown according to an unvarying law; and also that he held the opinion that the interior of the earth was fluid. According to this assumption his conclusion might not be far from correct, because the condition requisite for a uniform rate of increase is simply that a fixed temperature should be maintained at a fixed depth. Now, if the interior of the earth were fluid, and affected by convective currents, such would be very approximately true. For a fall of temperature could not take place at the fixed depth without equally taking place throughout the fluid nucleus. And it is obvious that an enormous lapse of time would be necessary to reduce the temperature of so vast a reservoir, by any sensible amount, by means of the small secular loss which takes place through the outer crust. Although, then, the sarcasm is not without point, that geologists as a race employ no higher method in their calculations than the "Rule of Three," nevertheless, upon the hypothesis probably in this geologist's mind, that was indeed the proper rule to use, and the conclusion would have been correct that the central fluid would be found at some depth within the limits named.

Sir W. Thomson's mode of reckoning the rate of increase proceeds upon a different plan. From astronomical and tidal considerations he has satisfied himself that the earth has not a fluid nucleus, and, from his mode of viewing the manner of its consolidation, he believes that it passed from the state of a fluid to that of a solid globe in a comparatively short space of time; since which period all geological events have happened. He has adapted a mathematical formula to express the rate of increase of temperature, at any given depth, in terms of the length of time since this complete consolidation took place; and by means of this formula he performs those seemingly marvellous computations which are so astonishing to the uninitiated. Now, it is a remarkable fact that, for reasonably small values of the time in question,—in other words, of the age of the habitable world,—and for all such depths as the puny efforts of man have been able to reach, it would be impossible to perceive any deviation from a uniform rate of increase. But

if it were possible to reach such depths as the geologist ventured to speak of, the rate, on Sir W. Thomson's hypothesis, would be found to be sensibly diminishing, and, even in the centre of the earth, a greater temperature need not exist than what he expected to find at 60 miles. The greater the value we assign to the presumed age of the world, the further down we should have to dig before we should find the rate sensibly diminishing. Some interesting particulars upon the relation between different assumed values for the world's age, and the depths at which it might be practicable to detect a diminution in the rate of increase of temperature, will be found in the address referred to. But the conclusion at which, upon the whole, we must arrive is that—on the supposition that the heat of the earth's crust comes up from below (and is not generated, as Prof. Mohr supposed, within itself)—no mine nor bore-hole is ever likely to be put down deep enough to solve the question whether the earth is solid from surface to centre, or not so. If ever it could be conclusively proved, by observation, that the rate began to diminish, that would be an argument that the earth is a solid cooling by conduction from its entire mass. But the shortest age which can be assigned to the world's history is too long to allow us to expect that such a depth will ever be reached.

The object of the portion of this article which relates to the observations at Sperenberg has been to demonstrate that the diminution in the rate of increase supposed to exist at that place was only apparent, and could be accounted for without assuming any such diminution of the rate to exist in the rock itself.

As a fact, however, the geologist has to deal only with the surface phenomena of the globe. It is a matter beyond his immediate province to enquire what may be the condition of the earth's interior, except to just such an extent as that must influence the surface conditions. Accordingly he is ready to accept from the physicist such conclusions as can be arrived at solely by methods outside his own province of study. But he asks the physicist, in return, not to ignore the well-ascertained conclusions of geology, nor to suppose that the mistake must certainly be on the geological side if they do not square entirely with his own results. The writer would beg to be allowed to submit that there are some things which do not well agree with Sir W. Thomson's conviction that the earth became solid throughout, and has since cooled as a solid; for such a supposition will not account for the phenomena of surface inequalities, as geologists have almost unanimously agreed to interpret them.

It is almost universally believed that the inequalities of the earth's surface were produced, and are maintained against degradation, by the contraction of its volume. The writer some time ago made a calculation* of the magnitude which these inequalities might be expected to have assumed, on Sir W. Thomson's view, expecting fully that a cooling solid earth would account for them all, and for losses by denudation besides; but to his surprise they came out far smaller than those actually existing. The elevated tracts and the ocean bottoms present deviations from a spherical surface much greater than could arise from a cooling solid sphere. Capt. Dutton, an American geologist and physicist, was led, by a similar consideration, to abandon the contraction theory altogether, and to seek for some other explanation of the formation of the inequalities. In short, it seems clear that if Sir William Thomson's views upon this point are correct, then the inequalities of the earth's surface are not due to the contraction of the sphere, and the plications of the strata must be accounted for in some inexplicable way. If, on the other hand, the inequalities of the surface are due to contraction, there is strong reason to doubt that the earth became solid throughout, and that it is now cooling as a solid, according to Sir W. Thomson's law.

There is another fact connected with mountain chains which has an important bearing upon this question. To adopt Prof. Green's description of these:—"They owe their superior elevation to the fact that the rocks of which they are composed have been squeezed and ridged up to a greater height than the rocks of the country on either side."† Now this peculiarity, as was pointed out by Capt. Dutton, and also implied by the writer‡ in his argument against Mr. Mallet's theory of volcanic energy, necessitates a "slip" of the earth's superficial strata over whatever it be that underlies them. It is obvious that a more or less perfectly fluid substratum is requisite to account for this peculiar character of mountain-chains. What this fluid condition may be, whether permanent or transient, whether it is connected with volcanic action or not, are questions open to discussion. But there must be—if not permanently, at least during epochs of the mountains rising—some such condition present. Indeed the nearest analogy to the formation of a mountain-chain seems to be found in the crushing together of the adjacent edges of two sheets of floating ice, the flotation of the sheets being a necessary part of the

* Cambridge Phil. Transactions, 1873.
† Geology for Students and General Readers, p. 462.
‡ Phil. Mag., October, 1875, p. 9.

conditions. It was from noticing the ridges thus formed in ice that the writer originally took his ideas upon this class of geological questions.

The writer's own theory, which he is proud to say was approved by the late Mr. Scrope very shortly before his death, is, that there exists such a stratum in a state of igneo-aqueous fusion, and that it is to the steam escaping from this, and so diminishing its volume, that the contraction of the globe and the consequent compression of the crust are mainly due. He thinks it probable that at least a large portion of the water now composing the oceans has by this means been transferred from the under to the upper side of the solid crust. How thick this lubricating layer may be he does not undertake to say, nor how deep down. If, however, such does exist, there will be probably some change in the physical relation of the substance of the earth as regards heat, at the surface of this stratum, such as would render the laws of the simple conduction of heat out of a hot solid globe towards the cold of space inapplicable to found upon them any theory as to the age of the world, or of the distribution of heat in its interior, based upon the observed rate of increase of temperature in the crust.

III. MOVEMENTS OF JUPITER'S CLOUD-MASSES.

By Richard A. Proctor.

IF Jupiter be regarded as a planet resembling our earth in condition, we find ourselves compelled to believe that processes of a most remarkable character are taking place on that remote world. It is singular with what complacency the believers in the theory that all the planets are very much alike accept the most startling evidence respecting disturbances to which some among those brother worlds of ours must needs on that hypothesis have been subjected. Mighty masses of cloud, such as would suffice to enwrap the entire globe on which we live, form over large regions of Jupiter or Saturn, change rapidly in shape, and vanish, in the course of a few minutes; and many are content to believe that what has thus taken place resembles the formation, motion, and dissipation of our own small clouds, though the sun pours but about a twenty-seventh part of the heat on

Jupiter, and but about a hundredth part on Saturn, which we receive from his rays. The outline of Jupiter, as indicated by the apparent position of a satellite close to his disc, expands and contracts through thousands of miles, yet the theory that Jupiter is still intensely hot must not for a moment be entertained, though the expansion and contraction of the solid crust of a cool planet through so enormous a range would vapourise a portion of its mass exceeding many times the entire volume of our earth. Saturn is seen by Sir W. Herschel and Sir J. Herschel, by Sir G. Airy, Coolidge, the Bonds, and a host of other observers, to assume from time to time the square-shouldered aspect, a change which—to be discernible from our distant standpoint —would imply the expansion and contraction of whole zones of Saturn's surface through 4000 or 5000 miles at least; yet it is better to believe that these stupendous changes have affected the solid crust of a planet like our earth, than to admit the possibility that the outline we measure is not that of the planet itself, but of layers of cloud raised to a vast height in the deep atmosphere surrounding a planet still glowing with its primeval fires.

The phenomena I am now about to consider belong to the same category. They are utterly inexplicable, or only explicable by the most sensational assumptions as to the processes taking place on Jupiter, if we adopt the old theory of Jupiter's condition; while if we regard Jupiter as an intensely-heated planet surrounded by and entirely concealed within a cloud-laden atmosphere several thousand miles in depth, they at once admit of the most simple and natural explanation.

It has, of course, long been known that the belts of Jupiter are phenomena of his atmosphere, not of his surface. The belts of lightest tint have been regarded as belts of cloud, and the darker belts as either the real surface of the planet seen between the cloud-belts, or else as lower cloud-layers, appearing darker because in shadow. Accordingly, when features of the belts have been watched in their rotational circuit, it has been clearly recognised that the rotation determined in this way is not necessarily or probably the true rotation of the planet itself. Further, it has been proved, beyond all possibility of question, that some at least among the spots upon the planet's belts have a motion of their own; for whenever two spots in different Jovian latitudes have been observed, it has been almost constantly noticed that the one nearer the Equator has had a greater rotation rate than the other. Again, it has sometimes

happened that instead of two spots, in different latitudes, a well-defined dark streak or opening, having its two extremities in different latitudes, has remained long enough to be observed during several rotations of the planet. In these cases it has been observed that the end of the streak nearest the Equator has travelled fastest, not only absolutely, but in longitude, insomuch that the position of the streak has notably altered.

Thus, in February, 1860, Mr. Long, of Manchester, noticed across a bright belt an oblique dark streak. "Its position" (I quote from a paper of my own written six years ago, when as yet the theory now before us was in its infancy) "might be compared to that of the Red Sea on the globe of the earth, for it ran neither north and south nor east and west, but rather nearer the former than the latter direction. The length of this dark space—of this rift that is, in the great cloud-belt—was about ten thousand miles, and its width at least five hundred miles; so that its superficial extent was much greater than the whole area of Europe." It remained as a rift certainly until April 10th, or for six weeks, and probably much longer. It passed away to the dark side of Jupiter, to return again after the Jovian night to the illuminated hemisphere, during at least a hundred Jovian days; and assuredly nothing in the behaviour of terrestrial clouds affords any analogue to this remarkable fact. "This great rift *grew*, lengthening out until it stretched across the whole face of the planet, and it grew in a very strange way; for its two ends remained at unchanged distances from the planet's equator, but the one nearest to the equator travelled forwards (speaking with reference to the way in which the planet turns on its axis), the rift thus approaching more and more nearly to an east and west direction." The rate of this motion was perhaps the most remarkable circumstance of all. Mr. Baxendell, one of the observers of the rift, and one of our most experienced telescopists, thus describes the changes seen in the belt:—"Since Mr. Long first observed the oblique streak on February 29th, it has gradually extended itself in the direction of the planet's rotation, at the average rate of 3640 miles per day, or 151 miles per hour, the two extremities of the belt remaining constantly on the same parallels of latitude. The belt also became gradually darker and broader."*

Apart from the evidence afforded by this rift respecting

* Two pictures of this belt—as seen March 12th, 1860, and April 9th, 1860—will be found in my article on "Astronomy," in the Encyclopædia Britannica, vol. ii., p. 808.

the swift motions of the cloud-masses enwrapping Jupiter (for a velocity of 151 miles per hour exceeds that of the most tremendous hurricanes on our earth), it has always seemed to me that this one series of observations should suffice of itself to show that the phenomena of Jupiter's cloud-laden atmosphere are not due to solar action. For the rift itself continued, and the changes affecting it continued whether Jovian day was in progress or Jovian night. For one hundred Jovian days or more, and for one hundred Jovian nights, the great cloud-masses on either side of the rift remained in position opposite each other, slowly wheeling, but still continuing face to face, as their equatorial ends rushed onwards at a rate fourfold that of a swift train, even measuring their velocity only by reference to the ends remote from the equator, and regarding these as fixed. Probably the cloud-masses were moving still more swiftly with respect to the surface of the planet below.

Of course, it is just possible that a great dark rift, such as I have described, might appear thus to change in position without any actual transference of the bordering cloud-masses. Mr. Webb, speaking of a number of phenomena, of which those presented by the great rift of 1860 are but a few, says that "they prove an envelope vaporous and mutable like that of the earth, without, however, necessarily inferring" [? implying] "the existence of tempestuous winds: even in our own atmosphere, when near the dew-point, surprising changes sometimes occur very quietly: a cloud-bank observed by Sir J. Herschel, 1827, April 19, was precipitated so rapidly that it crossed the whole sky from east to west at the rate of at least 300 miles per hour; and alterations far more sudden are conceivable where everything is on a gigantic scale." It does not seem to me altogether probable that more rapid alterations would affect cloud-banks covering millions of square miles, than occasionally affect terrestrial cloud-banks covering perhaps a few tens of thousands of square miles; on the contrary, as small terrestrial clouds change relatively in a far more rapid way than large ones, and these than cloud-masses covering a county or a country, so it would seem that the changes affecting our largest cloud-layers would be relatively far more rapid than those affecting cloud-masses which could (many times over) enwrap the whole frame of this earth on which we live. But apart from that, and apart also from the important consideration that all such processes as evaporation and condensation, so far as the sun brings them about should proceed far more sluggishly in the case of a

planet like Jupiter than in that of our earth, which receives some twenty-seven times as much heat from the sun (mile for mile of surface) it is utterly incredible that precipitation should have occurred so steadily and swiftly along one edge of the great rift, and condensation—with such exactly equal steadiness and swiftness—on the opposite edge, that, while the rift as a whole shifted its position during a hundred Jovian nights and days at the rate of 150 miles per hour, its sides should nevertheless remain parallel all the time. Such processes may be spoken of as possible, in the same sense that it is possible that a coin tossed fifty times in succession should always show the same face; but we do not reckon such possibilities among scientific contingencies.

But the motion of great rounded masses in the atmosphere of Jupiter is still more decisive as to the existence not only of a very deep atmosphere, but also as to the swift motions taking place in that atmosphere.

I would, in the first place, note that the very existence of belts in the Jovian atmosphere, and especially of variable belts, implies the great depth at which the real surface of the planet must lie below the visible cloud-layers. Atmospheric belts can only be formed where there are differences of rotational velocity. In the case of our own earth we know that the trade-wind zone and the counter-trade zone owe their origin to the difference of absolute rotational velocity between the equatorial parts of the earth and parts in high latitudes. In the case of Jupiter the difference of this kind is not sufficient to account for the observed belts,—partly because there are many, partly because they are variable, but principally because Jupiter is so much larger than the earth that much greater distances must be traversed in passing from any given latitude to another where the rotational velocity is so many miles per hour more or less. Combining with these considerations the circumstance that the solar action which causes the atmospheric movements from one latitude to another in the case of our earth, is reduced to one twenty-seventh part only of its terrestrial value in the case of Jupiter, we must clearly look to some other cause for the difference of absolute rotational velocity necessary to account for the belts of Jupiter.

Now, it seems to me that we are thus at once led to the conclusion that the cloud-masses forming the belts of Jupiter are affected by vertical currents, uprushing motions carrying them from regions nearer the axis, where the absolute motion due to rotation is slower, to regions farther from the axis, where the motion due to rotation is swifter, and

motions of downrush carrying them from regions of swifter to regions of slower rotational motion. This view seems certainly encouraged by what we find when we come to study more closely the aspect of the Jovian belts. The white spots—some small, some large—which are seen to form from time to time along the chief belts present precisely the appearance which we should expect to find in masses of vapour flung from far down below the visible cloud-surface of Jupiter, breaking their way through the cloud-layers, and becoming visible as they condense into the form of visible vapour in the cooler upper regions of the planet's atmosphere. Then again, the singular regularity with which in certain cases the great rounded white clouds are set side by side, like rows of eggs upon a string, is much more readily explicable as due to a regular succession of uprushes of vapour, from the same region below, than as due to the simultaneous uprush of several masses of vapour from regions set at uniform distances along a belt of Jupiter's surface. The latter supposition is indeed artificial and improbable in the highest degree, and in several distinct respects. It is unlikely that several uprushes should occur simultaneously, unlikely that regions whence uprush took place should be set at equal distances from each other, unlikely that they should lie along the same latitude parallel. On the other hand, the occurrence of uprush after uprush from the same region of disturbance, at nearly uniform intervals of time, is not at all improbable. The rhythmical succession of explosions is a phenomenon, indeed, altogether likely to occur under certain not improbable conditions,—as, for instance, when each explosion affords an excess of relief, if one may so speak, and is therefore followed by a reactionary process, in its turn bringing on a fresh explosion. Now, a rhythmical succession of explosions from the same deep-rooted region of disturbance would produce at the upper level, where we *see* the expelled vapour-masses (after condensation), a series of rounded clouds lying side by side. For each cloud-mass—after its expulsion from a region of slow absolute rotational motion, to a region of swifter motion—would lag behind with reference to the direction of rotational motion. The earlier it was formed the farther back it would lie. Thus each new cloud-mass would lie somewhat in advance of the one expelled next before it; and if the explosions occurred regularly, and with a sufficient interval between each and the next to allow each expelled cloud-mass to lag by its own full length before the next one appeared, there would be seen precisely such a series of

egg-shaped clouds, set side by side, as every careful observer of Jupiter with high telescopic powers has from time to time perceived.*

That these egg-shaped clouds are really egg-shaped—not merely oval in the sense in which a flat elliptic surface is oval—is suggested at once by their aspect. But it is more distinctly indicated when details are examined. It appears to me that considerable interest attaches to some observations which were made by Mr. Brett, in April, 1874, upon some of the rounded spots then visible upon the planet's equatorial zone. It will not be thought that I am disposed, as a rule, to place too much reliance upon the observations and theories of Mr. Brett, seeing that on more than one occasion I have had to call attention to errors into which, in my judgment, he has fallen. For instance, I certainly do not think he has ever seen the solar corona when the sun was not eclipsed, though I have no doubt he saw what he described, which he supposed to be the corona, but which was in reality not the corona. Nor, again, do I accept (though I do not think it worth while to discuss) his theory that Venus has a surface shining with metallic lustre, and is surrounded by a glassy atmosphere; though in that case, again, his description of what he saw may be accepted as it stands, and all that we need reject is his interpretation thereof. In the case of Jupiter's white spots, Mr. Brett's skill as an artist enables us to accept not only his observations, but his interpretation of them, simply because the interpretation depends on artistic, not on scientific, considerations.

"I wish," he says, "to call attention to a particular feature of Jupiter's disc, which" [the feature probably] "appears to me very well defined at the present time, and which seems to afford evidence respecting the physical condition of the planet. The large white patches which occur on and about the equatorial zone, and interrupt the continuity of the dark belts, are well known to all observers, and the particular point in connection with them to which I beg leave to call attention is *that they cast shadows;* that is to

* Webb thus describes the egg-shaped clouds:—"Occasionally the belts throw out dusky loops or festoons, whose elliptical interiors, arranged lengthways and sometimes with great regularity, have the aspect of a girdle of luminous egg-shaped clouds surrounding the globe. These oval forms, which were very conspicuous in the equatorial zone (as the interval of the belts may be termed) in 1869-70, have been seen in other regions of the planet, and are probably of frequent occurrence. The earliest distinct representation of them that I know of is by Dawes, 1851, March 8, but they are perhaps indicated in drawings of the last century."

say, the light patches are bounded on the side farthest from the sun by a dark border shaded off softly towards the light, and showing in a distinct manner that the patches are projected or relieved from the body of the planet. The evidence which this observation is calculated to afford refers to the question whether the opaque body of the planet is seen in the dark belts or the bright ones, and points to the conclusion that it is not seen at all in either of them, but that all we see of Jupiter consists of semi-transparent materials. The particular fact from which this inference would be drawn is, that the dark sides of the suspended or projected masses are not sufficiently hard or sharply defined for shadows falling upon an opaque surface; neither are they sharper upon the light background than upon the dark. The laws of light and shade upon opaque bodies are very simple and very absolute; and one of the most rudimentary of them is that every body has its light, its shade, and its shadow, the relations between which are constant; and that the most conspicuous and persistent edge or limit, in this association of elements, is the boundary of the shadow; the shadow being radically different from the shade, in that its intensity is uniform throughout in any given instance, and is not affected by the form of the surface on which it is cast, whereas the shade is distinguished by attributes of an opposite character. Now, if the dark spaces adjoining the light patches on Jupiter, which I have called shadows, are not shadows at all, but shades, it is obvious that the opaque surface of the planet on which the shadows should fall is concealed; whereas if they are shadows their boundaries are so soft and undefined as to lead to the conclusion that they are cast upon a semi-transparent body, which allows the shadow to be seen indeed, but with diminishing distinctness towards its edge, according to the acuteness of its angle of incidence. Either explanation of the phenomenon may be the true one, but they both lead to the same conclusion, viz., that neither the dark belts nor the bright ones are opaque, and that if Jupiter has any nucleus at all it is not visible to us. [It is obvious that the phenomena I have described would not be visible at the time of the planet's opposition, and the first occasion on which I noticed it was the night of the 16th of April last]."

This reasoning, so far as it relates to the laws of light and shade and shadow, is of course altogether sound. Nor are there any points requiring correction which in any degree affect the astronomical inferences deducible from what Mr. Brett actually saw. I may note that somewhat later

Mr. Knobel observed the shadow of white cloud-masses, and as the shadow had not so much greater a length at that time, two months from opposition, as it had when the planet was much nearer opposition, he infers that the true explanation of the appearance has hardly been found. He appears to have overlooked the fact that the assumption made in the explanation is not that Jupiter has a semi-transparent atmosphere always equally translucent and penetrable to the same depth by the solar rays. When the shadow was shorter than it should have been, had the atmosphere been in the same condition as when Mr. Brett made his observation, it is probable that a layer of clouds interrupted the rays, and thus the shadow was much closer to the cloud-mass throwing it than it would have been had that layer not been there. Mr. Knobel's paper contains very striking evidence of the variability of Jupiter's atmosphere, or rather of the clouds which float in it. "The greater distinctness of the satellites when near the edge," he says, "is a curious phenomenon which has been repeatedly observed by astronomers, but which seems to require explanation." On an occasion described "the second satellite transited a dark limb which was" [seemed] "most dark near the centre, and fainter towards the edge, yet the satellite was almost invisible when on the darkest part of the belt, and was bright and distinct when the background of the belt was faintest." This practically proved that on the occasion in question the dark central part of the belt seemed darker than it really was, by contrast with the bright belts on either side, while the edge seemed lighter than it really was, by contrast with the dark sky on which the planet was projected. In reality the part near the edge must have been darker than the part near the middle, or the satellite could not have appeared brighter when near the edge. No doubt the darkness near the edge (which, by the way, my friend Mr. Browning tested photometrically, and demonstrated, at my suggestion, eight years ago) was due to transparency, the darkness of the sky beyond being to some degree discernible through the edge. But this transparency is not always to be observed to the same degree, or through the same extent of Jovian atmosphere as to depth. Mr. Knobel proceeds, illustrating this the more effectively that he does so unintentionally:—"The third satellite, on March 25, 1874, appeared as a dark spot when in mid-transit, and on nearing the edge appeared as a bright spot without trace of duskiness. But on March 26, 1873" (observe the difference of years), "the fourth satellite made the whole transit as a dark spot,

and was not perceptibly less dark at egress than in mid-transit."

It appears to me demonstrated by the evidence thus far noted, that in a semi-transparent atmosphere of enormous depth, surrounding Jupiter, there float vast cloud-masses, sometimes in layers, at others in irregular heaps, at others having well-rounded forms. These cloud-masses undergo sometimes remarkable changes of shape, often forming or disappearing in a very short time, and thus indicating the inferior activity of the forces at work below them,—in other words, the intense heat of Jupiter's real globe. As to the actual depth of the semi-transparent atmosphere in which these cloud-layers and cloud-masses float, it would be difficult to express an opinion. We do not know how many cloud-layers there are, how thick any cloud-layer may be, how great may be the depth of the vast rounded masses of cloud whose upper surface (that is, the surface remotest from Jupiter's true surface) we can alone see under favourable conditions. But we can indicate a minimum than which the atmosphere's depth is probably not less; and from all the observations which I have examined as bearing on this point, I should be disposed to assign for that minimum at least 6000 miles. I am strongly of opinion that in reality the depth of the Jovian atmosphere is still greater. I cannot doubt that Jupiter has a solid or liquid nucleus, though this nucleus—glowing, as it must be, with a most intense heat—may be greatly expanded; yet I should conceive that, with the enormous attractive power residing in it, containing as it must nearly the whole mass of the planet, its mean density cannot be less than that of the earth. Now a globe of the mass of Jupiter, but of the same mean density as our earth, would have one-fourth of Jupiter's volume—the mean density of Jupiter, as at present judged, being equal to one-fourth that of the earth. The diameter therefore of such a globe would be less than the present diameter of Jupiter in the same ratio that the cube root of unity is less than the cube root of 4, or as 1 is less than 1·5874. Say, roughly, (remembering that the atmosphere of Jupiter must have a considerable mass) the diameter of Jupiter's nucleus would, on the assumptions made, be equal to about five-eighths of his observed diameter, or about 53,000 miles. This is less than his observed diameter by about 22,000 miles, so that the radius of his nucleus would be less than his observed radius by about 11,000 miles, which therefore would be the probable depth of his atmosphere.

But we have still to consider the velocities with which

rounded masses of cloud travel in the very deep atmosphere of Jupiter. "There is clear evidence," I have pointed out in the article 'Astronomy' of the "Encyclopædia Britannica," "that spots on Jupiter are subject to a proper motion like that which affects the spots on the sun. Schmidt, in No. 1973 of the 'Astronomische Nachrichten, gives a number of cases of such proper movements of spots, ranging in velocity from about 7 miles to about 200 miles an hour. It may be noted, also, that from a series of observations of one spot, made between March 13 and April 14, 1873, with the great Rosse reflector, a period of 9 h. 55 m. 4 s. was deduced, while observations of another spot in the same interval gave a rotation period of 9 h. 54 m. 55·4 s." The actual difference of velocity would depend in this case on the actual latitudes of the two spots, which were not micrometrically measured. Taking 200,000 miles as about the circumference of a parallel of latitude passing midway between the spots (only a very rough calculation need be made), we should find that in a period of one rotation, or roughly of ten hours, one spot gained on the other about 51 seconds, or roughly about 1-700th part of a rotation—that is, in distance (dividing 200,000 by 700) about 286 miles in ten hours, or nearly 29 miles an hour.

We have, however, instances of yet greater relative proper motion among cloud-masses. One of these cases I proceed to consider at length.

In June, 1876, two spots were visible upon the disc of Jupiter, so distinct and isolated as to be well adapted for measurement to determine the rate of the planet's rotation. Mr. Brett, observing them first as illustrative of the phenomenon to which he had called attention in 1874, turned his attention afterwards to their rate of motion. He would seem not to have been aware of the fact that the proper motion of bright spots and other markings on Jupiter was already a recognised phenomenon; for he asks whether his "observations of these spots, forming a series extending over a period of 286 hours 20 minutes, afford evidence of proper motion, or whether, on the other hand, they tend to cast any doubt on the accepted rotation of the planet." However, his observations are all the freer from the bias of preconceived opinions. "There were several peculiarities about these two spots," he says, "which seemed to me to give them an eminent claim to attention. They occurred very near to the equator, and were very well defined, and free from entanglement with other markings—an advantage which they have maintained with singular uniformity

throughout the period mentioned; but the special peculiarity to which attention is asked is that during an interval of five days they remained in the same relative position without any variation whatever. Their stability in respect of latitude during those five days is undoubted; but the question is whether or not they were equally stable in longitude. This remark only applies to the first five days of the series, because at the end of twelve days a certain deviation was obvious. The distance between the two spots occupied about 42 degrees of Jovian longitude, or about 33,000 miles. Their diameter is nearly equal, being estimated at about one-fourteenth of the planet's diameter, or 6310 miles." The interval of time between these first two observations "was 119 hours,—that is to say, twelve rotations of the planet according to Airy's determination, during which time their distance apart and their latitude remained constant." Between the first and second observations the two spots had gained "44 m. 6 s. in time. Assuming Airy's rotation, viz., 9 h. 55 m. 21 s., the spots have gained on the planet's surface at the rate of 4 m. 2 s. in each revolution."

Between the second observation and the third "there was an interval of seven days, or seventeen rotations of the planet; and the same two spots turn up again somewhat earlier than the calculated time. It unfortunately happens," proceeds Mr. Brett, "that on this occasion their configuration had undergone some change; but their dimensions and the distance between them remain very much as before. The most important circumstance respecting them is, that their rate of progress shows a certain acceleration." The change, however, in these seven days, is not such as to permit us to believe that the same pair of spots was under observation. *If* so, a change in latitude much more remarkable than the change in longitude had taken place; for the one which was the most northerly by about 6000 miles at the beginning of the seven days was the most southerly by nearly the same amount at the end of that period. Considering that in the five days between the first and second observations no change of latitude took place, it may fairly be doubted whether a change of the kind and so rapid—amounting, in fact, to nearly 900 miles per day, could have taken place in the interval. Proper motions in latitude may indeed be regarded as not less likely to occur in the case of Jupiter than in that of the sun, where they certainly sometimes occur; but all the observations hitherto made on Jupiter assure us that, in his case as in the sun's, proper motions in latitude would be very much slower than proper motions in

longitude. We must be content with the evidence of proper motion afforded by the first five days of observation. (The fourth observation only followed the third by about twenty minutes.)

Now, taking this evidence as it stands, and making fair allowance for probable error in an observation of the sort, we may consider that during the 119 hours the two spots were gaining on the estimated rotation-period of the planet by about four minutes per rotation. As they both lie on the equatorial belt, we may take the circuit accomplished by each at about 267,000 miles, or their rate at about 270,000 in ten hours, or 27,000 miles per hour. Hence the distance traversed in four minutes would be about 1800 miles, which would be about the gain per rotation. One-tenth of this, or 180 miles, would be the hourly gain, as compared with the estimated rotation-rate. Mr. Brett takes the least proper motion at 165 miles per hour.

He points out justly that the rotation-rate has been derived from observations of some such spots. So that in reality the only inference we can form is, that the rotation-rate derived from some spots is different from the rotation-rate derived from others, and that some spots (if not all) are certainly not constant in position with respect to the solid nucleus of the planet. That the spots observed by Airy, Mädler, and others should have indicated a slower rate of rotation than those observed by Mr. Brett may fairly be ascribed to the fact that the former were at some distance from the equator, while these last were nearly equatorial. For matter thrown up from the equatorial parts of the true surface of the concealed planet would manifestly differ less in velocity from the superincumbent atmosphere into which they were driven than would masses expelled from higher latitudes. (It is probable that the same explanation applies also in the case of the sun.)

This conclusion, that the spots of Jupiter have rapid rates of relative motion, would of itself be of singular interest, especially when we remember that the larger white spots represent masses of cloud 5000 or 6000 miles in diameter. That such masses should be carried along with velocities so enormous as to change their positions relatively to each other, at a rate sometimes of more than 150 miles per hour, is a startling and stupendous fact. But it appears to me that the fact is still more interesting in what it suggests than in what it reveals. The movements taking place in the deep atmosphere of Jupiter are very wonderful, but the cause of these movements is yet better worthy of study.

We cannot doubt that deep down below the visible surface of the planet—that is, the surface of its outermost cloud-layers—lies the fiery mass of the real planet. Outbursts, compared with which the most tremendous volcanic explosions on our earth are utterly insignificant, are continually taking place beneath the seemingly quiescent envelope of the giant planet. Mighty currents carry aloft great masses of heated vapour, which, as they force their way through the upper and cooler strata of the atmosphere, are converted into visible cloud. Currents of cool vapour descend towards the surface, after assuming no doubt vorticose motions, and sweeping away over wide areas the brighter cloud-masses, so as to form dark spots on the disc of the planet. And owing to the various depths to which the different cloud-masses belong, and whence the uprushing currents of heated vapour have had their origin, horizontal currents of tremendous velocity exist, by which the cloud-masses of one belt or of one layer are hurried swiftly past the cloud-masses of a neighbouring belt or of higher or low cloud-layers. The planet Jupiter, in fact, may justly be described as a miniature sun, vastly inferior in bulk to our own sun, inferior to a greater degree in heat, and in a greater degree yet in lustre, but to be compared with the sun—not with our earth—in size, in heat, and in lustre, and lastly in the tremendous energy of the processes which are at work throughout his cloud-laden atmospheric envelope.

Since the above article was written news has been received by the Astronomical Society that Mr. Todd, a well-known observer of Adelaide, New South Wales, has been able to trace the motions of satellites behind the parts of the planet near the edge, or, in other words, *through* those parts of the planet's atmosphere which have hitherto been regarded as belonging to the mass of the planet itself.

IV. NATIONAL WEALTH AND PUBLIC DEBT.

By FRED. CHAS. DANVERS.

FEW people, probably, are aware of the vast amount of information that is contained in the several "Command" Papers annually laid before Parliament, and published as Parliamentary Blue Books. Many of them are doubtlessly very dry reading, *per se*, but it is the object of the present article to extract from many hundreds of pages of figures and tabular statements, contained in printed Parliamentary Papers, and from other sources, information on certain points which can hardly fail to interest any "true-born Englishman," and which, being taken or compiled principally from official sources, is not likely to be otherwise than reliable.

It is quite possible to contemplate mentally, in the space of a few moments, on the early prosperity and subsequent decline of Egypt; on the rise and fall of Israel, Assyria, Persia, Macedonia, Greece, and Rome; also on the slow but gradual growth of bands of wandering Scythians and Goths into the mighty empires which now inhabit the North and North-West of Europe, the whole of the North-American Continent, Australasia, and innumerable islands scattered all over the globe. These are all matters of history, which, having been carefully recorded from time to time by cotemporaneous authors, handed down by tradition, or traced by the discovery of monuments or other national emblems, have formed subjects for investigation and study, the results of which are within the reach of every schoolboy. To reflect deliberately, however, upon the circumstances that have led to such violent political changes as have transferred the seat of wealth, learning, and influence from the East to the West, cannot but fill the mind with thoughts calculated to excite feelings of wonderment, if not of awe. Are the changes that have taken place within the past two thousand years due to the action of forces still in existence? and, if so, to what will they ultimately lead? are questions that may very naturally occur to any thinking mind, but it will be by no means easy to afford satisfactory replies to them.

The growth of wealth naturally takes place together with an advance of civilisation in any nation. The former, however, has a natural tendency to encourage luxury and indolence, and to impair that spirit of energy and enterprise

which is necessary to build up a nation. With these prosperity has a tendency to decline, and when once they have become ruling passions in any race its decadence must of necessity speedily follow. To the fact of this Ancient Greece and Rome abundantly testify.

Two of the leading elements of national prosperity are the encouragement of colonisation and foreign trade. Greece and Rome were both of them at one time the pioneers in commerce and in the establishment of foreign settlements. In more modern times Holland, Spain, and Portugal were conspicuous amongst nations for the same reasons, and the names of Sebastian and John Cabot, Tasman, Bartholomew de Diaz, Vasco de Gama, and Columbus are well known to all readers of history, but the lands discovered and taken possession of by them, in the names of their respective Governments, have, for the most part, long since passed into other hands, and the glory and influence once possessed by the countries they represented has become a thing of the past.

In later times the task of colonisation has passed almost exclusively into the hands of the English, and the British flag now waves where formerly a tricolor was the standard representing the supreme power. Emigration from this country, of some description, and on a limited scale, began in the early part of the seventeenth century. Virginia, Massachusetts, the Bermudas, and Barbadoes were the fields in which the first successful attempts at colonisation by the English may be said to have commenced. On the 13th of May, 1607, the first colony in Virginia was planted under a patent granted in the preceding year by James I., to a London company, and subsequently settlements in the Bermudas and Barbadoes were effected in 1612 and 1625. Earlier attempts at founding British colonies had indeed been made by Sir Richard Grenville, Sir Walter Raleigh, and others, but they had failed. To those names, however, must be accorded the honour of having been the pioneers of that progress which has now, in a great measure, secured to England a position never possessed by any other nation.

It can hardly be questioned that the colonial policy of Great Britain during the past 270 years has been mainly instrumental, humanly speaking, in the establishment under the British crown, of the largest empire of either modern or ancient times, compared with which the Roman empire, in the zenith of its prosperity, was insignificant. The possessions of the British empire comprise an area of nearly eight and a half millions of square miles, and a population of upwards of 283 millions, scattered all over the world,

and treading British soil in Europe, Asia, Africa, America, and in most of the principal islands of the seas. The nearest approach to these figures is found in the Russian empire, the area of whose possessions is a very little short of eight millions of square miles, with a population, however, of scarcely 82 millions. Like the British empire, Russia is both an European and Eastern power, but, whilst the latter owns nearly an equal extent of territory, her population is less than one-third of that of the former. China has a smaller territory still, but her population is said to number upwards of 400 millions. There is, however, no official information available on this point, and figures relating to the Flowery Land must therefore be accepted only as estimates, and with caution.

Next in order comes the United States, with 3,600,000 square miles of territory, and 38½ millions of population. After this the territorial possessions of States rapidly decrease. Germany and France are nearly of a size, having each slightly over 200,000 square miles of possessions; but whilst the former has a population of 41 millions, the latter has only 36 millions. Austria and Hungary together comprise about 240,000 square miles, with a united population about equal to that of France. Spain is under 200,000 square miles in extent, and Italy is but 114,000, the populations of these two empires being about 17 millions and 27 millions respectively.

Whilst the British empire thus stands prominently first in extent, it is the only one that has been successful in planting colonies and maintaining them as integral parts of the empire. Those who foreran England in this respect have, as a rule, lost most of the colonial possessions once claimed by them, which have, for the most part, since fallen to the lot of England, whilst those yet retained by them are generally sources of weakness rather than of strength, as a proof of which we may witness Algeria, France's only colony, and Cuba, the principal remnant of Spain's foreign possessions. France has, at different times, resigned to England, either through conquest or by treaty, Canada, Nova Scotia, New Brunswick, Newfoundland, Prince Edward Island, St. Lucia, Grenada, Mauritius, Antigua, Dominica, Tobago, the Bahamas, and Malta. From the Dutch England has obtained the Cape Colony, British Guiana, Ceylon, and St. Helena; whilst from Spain she holds Gibraltar, Trinidad, Jamaica, and the Falkland Islands.

What—it may be asked by some—is the use to us of these colonies? Much, in many ways. It has been

ascertained, beyond doubt, that the English population increases at a more rapid rate than that of any other nation, and, were it not for emigration, and foreign possessions to which our surplus population can safely transfer themselves and their fortunes, the land would long since have become over-populated, and the means of livelihood more difficult by far than it now is. Besides this advantage, however, the command of the colonial trade which this country enjoys *through possession of the colonies*, affords no mean increase to its commercial business, and therefore to its profit. It has been shown, and proved by statistics, by the late Mr. C. W. Eddy, late Secretary to the Colonial Institute, in his "Tables of British Commerce," that "the Trade follows the Flag," or, in other words, that the mother country enjoys the greater portion of the trade of its colonies.

The trade between England and her colonies amounted in 1875 to a total declared value of £161,078,982, of which the exports of foreign and colonial produce, and manufactures from England to British possessions, were valued at £5,562,848; the exports of British and Irish produce at £71,092,163; and the value of imports into England of colonial produce and manufactures at £84,423,971. It will thus be seen that the British colonies do, in a large measure, contribute towards the trade, and therefore the means of wealth, of the mother country. And not only so; for when compared with foreign countries our colonies are very considerable consumers of British produce, as the following figures plainly show:*—

	Consumption of British Produce per Head.			Proportion of British Produce to Total Imports
	£	s.	d.	Per cent.
North American Colonies .	1	5	8	42
Australia and New Zealand .	8	10	3	47
Cape and Natal { Total population .	2	6	4	69
Natal { White population.	8	12	2	
West Indies	2	8	7	43
Mauritius	1	14	7	30
United States	0	12	10	31
France	0	6	0	9
Spain	0	2	1	18
Portugal	0	10	4	—
Germany	0	6	11	—

* HAMILTON, "On the Colonies." Paper read before the Statistical Society March 19th, 1872.

	Consumption of British Produce per Head. £ s. d.	Proportion of British Produce to Total Imports. Per cent.
Italy	0 4 3	17
Russia	0 0 11	17
Holland	2 16 2	10
Belgium	0 11 10	8
Brazil	0 11 2	—

There are not wanting people who, wholly overlooking the indirect pecuniary benefits to this country derived from the increase of trade secured in consequence of its colonies, and the power and political importance enjoyed by means of its widely-spread possessions, object that they should be in any way a direct pecuniary burden to the mother country. But are they any burden? The supposed cost of the colonies to the Imperial Treasury for the nineteen years 1853 to 1871 was £43,810,000, and during the same time the exports of British produce to the colonies were valued at £450,798,000 (see Archibald Hamilton's paper already referred to). Now the amount realised by the exports of home produce and manufactures constitute in reality so much of our aggregate income; and it has been variously estimated that in this country we are taxed from 10 per cent to as high as 20 per cent on our incomes. Taking the lowest estimate it follows that the Treasury has, during the nineteen years in question, obtained a revenue of £45,000,000 in consequence of the colonial trade, while the expenditure has not exceeded £43,810,000. Similarly it may be shown that for the year 1871—the last year of the above-mentioned nineteen years—the Treasury derived at least £2,580,000 from the colonial trade, and expended, £1,100,000.

Hence we see that England enjoys an increase of wealth, as well as of power and political importance, from the possession of colonies; and this is a source of national prosperity that is not enjoyed by any other nation, the small colonial possessions of Spain, Holland, and France not being of sufficient importance to be taken into account. The colonies and dependencies of Great Britain are, however, more than sixty-seven times in area the size of the United Kingdom.

It would be but little use having extensive colonies unless a large surplus population were forthcoming to people them. England is, of all countries, the best capable of supplying this want, for she is the most productive in increase of population. From some unexplained reason nations vary

greatly as to the relative quickness with which they double themselves, and it has been calculated that in England the population doubles itself in every 56 years; in the New World the Anglo-Saxons double in every 25 years. The Dutch double in 106 years; the Turks in 555 years; the Italians in 135 years; the Swedes in 92 years; the Russians in 100 years; the Spaniards in 112 years; the North Germans in from 50 to 60 years, and the South Germans in 167 years; whilst the French populations take about 140 years in which to double.

According to the last census (1871) the population of the entire British Empire was ascertained to be 234,762,593,* or nearly one-sixth of the total estimated population of the world. The territorial extent of the empire was, at the same time, found to be 7,769,499* square miles, comprising in the two hemispheres, Great Britain and Ireland, the surrounding islands in the British seas, parts of Europe, America, Africa, Asia, and Australasia.

As regards public revenue, the British Empire stands out unmistakably first amongst European nations, whilst the revenue of Great Britain is only exceeded by that of France and of the United States. The gross amount of public revenue of the British Empire amounts to £154,360,336, or nearly 13*s.* 2*d.* per head of population. The revenue of the United Kingdom is £77,131,693, or £2 7*s.* 1*d.* per head of population. The following table shows the same particulars with reference to the principal foreign nations. The figures here given, it must be observed, are exclusive of what may be termed provincial taxation.

	Revenue.	Amount per Head of Population.		
	£.	£	*s.*	*d.*
†France	86,125,625	2	7	8½
United States	77,939,000	2	0	5
Russia (including railways) .	72,437,000	0	17	8½
Austria and Hungary . . .	63,356,000	1	15	4
Prussia	53,568,000	1	6	1
Italy	47,226,000	1	15	2¾
Spain	21,502,000	1	5	7¾
Holland (including railways)	8,936,000	2	1	0
Belgium	8,534,000	1	15	3½
Portugal	5,766,000	1	8	8½

* Exclusive of Native States in India, which have been included in the figures before given.

† These figures are for the year 1871. The annual revenue of France is now upwards of one hundred millions.

From these calculations we perceive that the United Kingdom pays more revenue per head of population than any one of the other great powers except France. This alone does not prove that her wealth is greatest, but, taken in conjunction with other considerations which will presently be set forth, it will be seen that in this respect also she is the first amongst nations. The low average of taxation in the entire British Empire is accounted for by the extremely large numbers of her oriental population, which is characteristically poor, especially when compared with the purely British population. The ease with which the national revenues are respectively collected, and the burden borne by the population, show how far each country is living within its powers of taxation; but it will be seen that, whereas the revenues of the British Empire imposed and collected are based upon the necessary expenditure of the State, and prove sufficient —and more so generally—to meet outlay, other States, notwithstanding their relatively lower rates of taxation, fall far short of their requirements.

A close analysis of statistical facts shows that the Anglo-Saxon race are possessed of an elasticity of resources by reason of which they are enabled not only to meet their current expenditure, but they also are equal to any sudden emergency, and are able to bear, when necessary, a considerable increase of taxation, and to meet the requirements of the State caused by any contingency, out of their own resources; besides which they are the bankers of the whole world, and supply funds for the necessities of foreign States also.

With the exception of Russia, Spain, and Italy, the principal European States do manage to maintain an equilibrium in their finances. Austria, however, is sailing very close to the wind, whilst Turkey is hopelessly bankrupt. The power of keeping well within their means appears to belong almost exclusively to the Teutonic races of Northern Europe, if we except France, which, next to England and America, is perhaps really the wealthiest power in existence.

The peculiar situation of England, with reference to the rest of the world, added to her insular position and the possession of many accessible and safe ports, renders her especially well adapted to be the principal merchant and trader for the rest of the world; and by virtue of these and other necessary qualifications she has indeed become the chief carrier of the commerce of nations. The statistics from which we are quoting do not, it is true, show how much of the import and export trade of foreign countries is

carried in English vessels, but from the figures which will presently be given it will be clear that a large portion of the sea-borne trade of foreign countries must be carried in British ships.

The total of imports and exports of merchandise into and from the United Kingdom during the year 1875 amounted in value to £655,551,900, of which the imports were valued at £373,939,577, and the exports at £281,612,323. After England, France stands next highest on the list, her imports and exports having been £176,900,000 and £188,084,000 in value respectively, thus showing trade of an aggregate value of £364,984,000, or only a little more than half that of the United Kingdom. Next comes the United States, with a total trade valued at £240,350,000; then Belgium, with a trade valued at £183,587,000; Austria, with a trade of the value of £152,636,000; and Russia, with only £120,120,000, the value of the import and export trades of other kingdoms being still lower. Thus England, with regard to her imports and exports stands pre-eminently first amongst nations.

Let us now examine these figures, so far as they relate to the English trade, in a little further detail. Of the total imports, those from foreign countries were of the value of £289,515,606, and those from British possessions of the value of £84,423,971. Of this trade, foreign and colonial produce to the value of £58,146,360, or about one-sixth of the whole imported, was re-exported in its original state, whilst by far the greater part of the imports that was not reserved for home consumption, and consisting probably of raw materials, was manufactured in this country and re-exported as British produce. This fact alone will afford some idea of the importance of the manufacturing industries of this country.

Besides the above, however, the bullion trade of this country is by no means inconsiderable. The imports of gold and silver bullion and specie for the year 1875 amounted in value to £33,264,789, whilst the exports for the same time were valued at £27,628,042, from which it would appear that £5,636,747 in bullion and specie remained in the country, in exchange, no doubt, for goods of British production or manufacture of an equal value, which sum therefore appears to represent the balance of trade with foreign countries in favour of this country on the total transactions for the year.

So gigantic a trade as is represented by the above figures must necessarily require a by no means inconsiderable fleet

of vessels for its service. The total tonnage of vessels, of all nationalities and classes, entered and cleared at ports in the United Kingdom during the year, from and to foreign countries and British possessions, was 46,276,838 tons, of which upwards of two-thirds, or 30,944,744 tons, were British vessels.

The mercantile fleet of this country (exclusive of river steamers) consists of 17,221 sailing-vessels, having an aggregate tonnage of 4,044,504 tons, and 2970 steam-vessels, of 1,847,188 tons, or together 20,191 vessels, of a gross aggregate tonnage of 5,891,692 tons, on which are employed nearly 200,000 hands, exclusive of masters. Compared with these figures the mercantile marine of foreign countries is small indeed. Next after Great Britain the United States have the largest fleet. The vessels employed upon foreign trade aggregate 3,272,739 tons, and those employed in the river, lake, and home trade 1,423,288 tons, or together 4,696,027 tons. Next comes Norway, with 1,245,293 tons; then France, with 1,068,031 tons: Italy, with 1,046,439 tons, whilst other foreign countries have none of them more than about half the last-named tonnage each, and most of them considerably less. The tonnage of British merchant vessels is in excess of those of Norway, Sweden, France, Italy, Austria, Holland, Denmark, Hamburg, Bremen, Belgium, and Greece all put together.

Perhaps one of the most important sources of wealth to a nation is its mineral production. It is not intended now to compare the relative produce of different countries in coal and iron—the two most important products of mining industries; but we may notice a few circumstances in connection with the imports and exports of different countries. If we except the United States, France and Belgium alone of foreign countries possess any iron and coal industries of importance, whilst these form two of the most important industries of Great Britain; but gold, silver, and other more valuable metals abound chiefly abroad, and are imported into this country in their raw state. The exports of foreign countries consist chiefly of food supplies and raw materials, whilst of those from the United Kingdom manufactured articles largely predominate; and thus we see that England not only acts as the principal carrier, but also as the chief manufacturer for other nations; hence the wealth of the latter has a tendency to flow to this country.

Not only is coal produced in England in larger quantities than in any other part of the world, but there is also a general distribution of that mineral throughout the

possessions of the British Empire abroad; and when we consider how important coal is for the maintenance of supremacy on the seas, this appears to be a most important element both for commercial and naval success. As was observed by Mr. Alfred Eccles, in his Report on the New Zealand Exhibition of 1865, "It is a fortunate circumstance that, with few exceptions, wherever important British colonies have been founded, there has also been found a local supply of coal; thus the colonist of that race which, above all others, has attained great national prosperity by means of its vast mineral wealth, find in their new countries the same agents whereby to build up a like greatness." And, with regard to its importance to our steam navy and merchant vessels, the late Mr. Eddy, to whom we have already referred, has remarked—"Here, again, the singular fact which I have already referred to meets us, viz., that of the general absence of true coal fit for marine purposes, and also conveniently situated near the coast, from all except British territory. The fact is not only remarkable, but significant of the destiny of our race as the great maritime power of the world. During the whole voyage from England to India, up the Mediterranean, the Red Sea, and along the coast of Arabia, no native coal appears, as far as I am aware; and moreover, whenever you come to depôts of fuel you find it is of English origin, and generally, too, on English territory. Witness Gibraltar, Malta, Aden. Verily we hold the keys of the world, and long may we continue to hold them with a firm grasp. Until we reach India we meet with none but English coal, and there British coal from our own colonies at the Antipodes meets ours in the market."

Turning now to the national debts of different countries, it appears that Holland was the first to introduce the practice of raising public loans, and other nations have not been slow to follow the example. The public debt of this country may be said to date from the reign of James I., and, like other countries, it has chiefly been raised for the purpose of defraying the expenses of wars and of military preparations, although in more recent years many nations have added to their debts for the construction of railways, but these, in many instances, have been made more with a view to military and political than to commercial and peaceful requirements, present or prospective. It is perhaps a curious coincidence that England and France, the two most prosperous countries in Europe, have also the largest debts, exceeding in both cases upwards of £700,000,000 sterling, and they are also the only two upon whom the burden of

paying interest falls comparatively light. As regards England, her debt may be said to be entirely an "internal" debt, all monies required by the State being readily obtained at home. It is not so, however, with other countries, whose "internal" debts are, for the most part, of small amount, whilst the greater part of their loans have been obtained from this country. England, France, and the United States alone are enabled to take steps for the gradual extinction of their debts, whilst many other countries are obliged to raise fresh loans annually to meet their requirements for current expenses, or, failing that, to repudiate obligations to their bondholders, of the truth of which circumstance too many Englishmen have bitter cause of experience.

With regard to the amounts of public debts the following figures furnish such particulars as are obtainable. Column 1 shows the total public debts as they are given in the printed Parliamentary Papers, whilst Column 2 shows the amounts of the several external debts given in the "Investor's Monthly Manual," and which are quoted in the London Stock Exchange List :—

	£.	£.
Russia	296,096,000	146,549,128
Norway	1,766,000	—
Sweden	6,681,000	3,958,100
Denmark . . .	12,899,000	1,804,000
Germany . . .	140,404,000	—
Holland	*79,678,000	*79,801,240
Belgium	43,220,000	27,270,000
France	*731,338,000	*756,741,160
Spain	347,012,000	165,506,300
Portugal . . .	77,668,000	77,304,710
Switzerland . .	1,104,000	—
Austria	264,487,000	199,420,132
Hungary . . .	53,269,000	23,062,200
Italy	339,051,000	33,807,582
Greece	17,015,000	4,749,900
United States . .	469,427,000	338,576,803

If we attempt to draw any conclusion from the foregoing facts and figures, it is impossible to do otherwise than admit that England's supremacy is undeniable. That it is

* There is evidently some discrepancy in these figures, due either to typographical errors or to imperfect information on the part of the compilers of the respective returns from which the figures are taken.

acknowledged by other nations is conspicuous from the importance attached by them to her counsel and advice in political affairs. Wealth in individuals always has, and always will, command respect from its poorer neighbours, and what is true in individuals also holds good with regard to nations. At no time, probably, has the influence of England been more proved than recently. England is not only the greatest European power, but as an Eastern power, also, she holds a higher position than any hitherto recorded in history. The Macedonian and Roman Empires of old were small compared with that of Great Britain of the present day, and, so far as can be at present seen, her power and influence seem rather destined to increase than to diminish, whilst English is not only cultivated by other nations to an extent far beyond all past experience with regard to the Latin language,—formerly the most popular of tongues,—but it has been calculated that, at the present rate at which it is being spread, English will, a hundred years hence, be spoken by greater numbers than, according to the most reliable estimates, there are now people existing upon the face of the earth.

A glance at a telegraph map of the world will at once show how almost all the submarine cables connect British possessions, extending from New Zealand through Australia, the Straits Settlements, India, Aden, Malta, Gibraltar, to England, Ireland, and thence across the Atlantic to Newfoundland and Canada, by which all our most important possessions are placed in telegraphic communication with each other, whilst branch lines provide means of communication with other British territories and with most parts of the civilised world. These cables are all British property: and as they are required chiefly for commercial purposes, they are very properly possessed by that nation which claims the largest part of the commerce of the world.

V. "THE GREAT ICE AGE" AND THE ORIGIN OF THE "TILL."

By W. MATTIEU WILLIAMS, F.R.A.S., F.C.S.

THE growth of Science is becoming so overwhelming that the old subdivisions of human knowledge are no longer sufficient for the purpose of dividing the labour of experts. It is scarcely possible now for any man to become a naturalist, a chemist, or a physicist, in the full sense of either term; he must, if he aims at thoroughness, be satisfied with a general knowledge of the great body of science, and a special and full acquaintance with only one or two of its minor subdivisions. Thus geology, though but a branch of natural history, and the youngest of its branches, has now become so extensive that its ablest votaries are compelled to devote their best efforts to the study of sections which but a few years ago were scarcely definable.

Glaciation is one of these, which now demands its own elementary text-books over and above the monographs of original investigators. This demand has been well supplied by Mr. James Geikie in "The Great Ice Age,"* of which a second edition has just been issued. Every student of glacial phenomena owes to Mr. Geikie a heavy debt of gratitude for the invaluable collection of facts and philosophy which this work presents. It may now be fairly described as a standard treatise on the subject which it treats.

One leading feature of the work offers a very aggressive invitation to criticism. Scotchmen are commonly accused of looking upon the whole universe through Scotch spectacles, and here we have a Scotchman treating a subject which affects nearly the whole of the globe, and devoting about half of his book to the details of Scottish glacial deposits; while England has but one-third of the space allowed to Scotland, Ireland but a thirtieth, Scandinavia less than a tenth, North America a sixth, and so on with the rest of the world. Disproportionate as this may appear at first glance, further acquaintance with the work justifies the pre-eminence which Mr. Geikie gives to the Scotch glacial deposits. Excepting Norway, there is no country in Europe which affords so fine a field for the study of the vestiges of extinct glaciers as Scotland, and Scotland has

* The Great Ice Age, and its Relation to the Antiquity of Man. By JAMES GEIKIE, F.R.S., &c. Second edition, revised, 1877. Daldy and Isbister.

an advantage even over Norway in being much better known in geological detail. Besides this, we must always permit the expounder of any subject to select his own typical illustrations, and welcome his ability to find them in a region which he himself has directly explored.

Mr. Geikie's connection with the Geological Survey of Scotland has afforded him special facilities for making good use of Scottish typical material, and he has turned these opportunities to such excellent account that no student after reading "The Great Ice Age" will find fault with its decided nationality.

The leading feature—the basis, in fact—of this work deserves especial notice, as it gives it a peculiar and timely value of its own. This feature is that the subject—as compared with its usual treatment by other leading writers—is turned round and presented, so to speak, bottom upwards. De Saussure, Charpentier, Agassiz, Humboldt, Forbes, Hopkins, Whewell, Stark, Tyndall, &c., have studied the living glaciers, and upon the data thus obtained have identified the work of extinct glaciers. Chronologically speaking they have proceeded backwards, a method absolutely necessary in the early stages of the enquiry, and which has yielded admirable results. Geikie, in the work before us, proceeds exactly in the opposite order. Availing himself of the means of identifying glacial deposits which the retrogressive method affords, he plunges at once to the lowest and oldest of these deposits, which he presents the most prominently, and then works upwards and onwards to recent glaciation.

The best illustration I can offer of the timely advantage of this reversed treatment is (with due apology for necessary egotism) to state my own case. In 1841, when the "glacial hypothesis," as it was then called, was in its infancy, Prof. Jamieson, although very old and nearly at the end of his career, took up this subject with great enthusiasm, and devoted to it a rather disproportionate number of lectures during his course on Natural History. Like many of his pupils I became infected by his enthusiasm, and went from Edinburgh to Switzerland, where I had the good fortune to find Agassiz and his merry men at the "*Hotel des Neufchatelois*"—two tents raised upon a magnificent boulder floating on the upper part of the Aar glacier. After a short but very active sojourn there I "did," not without physical danger, many other glaciers in Switzerland and the Tyrol, and afterwards practically studied the subject in Norway, North Wales, and, wherever else an opportunity offered;

reading in the meantime much of its special literature; but, like many others, confining my reading chiefly to authors who start with living glaciers and describe their doings most prominently. When, however, I read the first edition of Mr. Geikie's "Great Ice Age," immediately after its publication, his mode of presenting the phenomena, bottom upwards, suggested a number of reflections that had never occurred before, leading to other than the usual explanations of many glacial phenomena, and correcting some errors into which I had fallen in searching for the vestiges of ancient glaciers. As these suggestions and corrections may be interesting to others, as they have been to myself, I will here state them in outline.

The most prominent and puzzling reflection or conclusion suggested by reading Mr. Geikie's description of the glacial deposits of Scotland was, that the great bulk of them are quite different from the deposits of existing glaciers. This reminded me of a previous puzzle and disappointment that I had met in Norway, where I had observed such abundance of striation, such universality of polished rocks and rounded mountains, and so many striking examples of perched blocks, with scarcely any decent vestiges of moraines. This was especially the case in Arctic Norway. Coasting from Trondhjem to Hammerfest, winding round glaciated islands, in and out of fjords banked with glaciated rock-slopes, along more than a thousand miles of shore line, displaying the outlets of a thousand ancient glacier valleys, scanning eagerly throughout from sea to summit, landing at several stations, and climbing the most commanding hills, I *saw only one ancient moraine*—that at the Oxfjord Station described in Chap. 6 of "Through Norway with a Knapsack."

But this negative anomaly is not all. The ancient glacial deposits are not only remarkable on account of the absence of the most characteristic of modern glacial deposits, but in consisting mainly of something which is quite different from any of the deposits actually formed by any of the modern glaciers of Switzerland or any other country within the temperate zones.

I have seen nothing either at the foot or the sides of any living Alpine or Scandinavian glacier that even approximately represents the "till" or "boulder clay," and have met with no note of this very suggestive anomaly by any writer on glaciers. Yet these are vast deposits, covering thousands of square miles even of the limited area of the British Isles, and which constitute the main evidence upon which we base all our theories respecting the existence and the vast extent and influence of the "Great Ice Age."

Although so different from anything at present produced by the Alpine or Scandinavian glaciers, this great deposit is unquestionably of glacial origin. The evidences upon which this general conclusion rests are fully stated by Mr. Geikie, and may safely be accepted as incontrovertible. Whence, then, the great difference?

One of the suggestions to which I have already alluded as afforded by reading Mr. Geikie's book was a hypothetical solution of this difficulty, but the verification of this hypothesis demanded a re-visit to Norway. An opportunity for this was afforded in the summer of 1874, during which I travelled round the coast from Stavanger to the Arctic frontier of Russia, and through an interesting inland district. The observations there made, and strengthened by subsequent reflections, have so far confirmed my original speculative hypothesis that I now venture to state it briefly as follows:—

That the period appropriately designated by Mr. Geikie as the "Great Ice Age" includes at least two distinct periods or epochs—the first of very great intensity or magnitude, during which the Arctic regions of our globe were as completely glaciated as the Antarctic now are, and the British islands and a large portion of Northern Europe were glaciated as completely, and nearly in the same manner, as Greenland is at the present time; that long after this, and immediately preceding the present geological epoch, there was a minor glacial period, when only the now existing valleys favourably shaped and situated for glacial accumulations were partially or wholly filled with ice. There may have been many intermediate fluctuations of climate and glaciation, and probably were such, but as these do not affect my present argument they need not be here considered.

So far I agree with the general conclusions of Mr. Geikie as I understand them, and with the generally received hypotheses, but in what follows I have ventured to diverge materially.

It appears to me that the existing Antarctic glaciers and some of the glaciers of Greenland are essentially different in their conformation from the present glaciers of the Alps, and those now occupying some of the fjelds and valleys of Norway; and that the glaciers of the earlier or greater glacial epoch were similar to those now forming the Antarctic barrier, while the glaciers of the later or minor glacial epoch resembled those now existing in temperate climates, or were intermediate between these and the Antarctic glaciers. The nature of the difference which I suppose to

exist between the two classes of glaciers is this:—The glaciers (properly so called) of temperate climates being the overflow of the *nevé*, or the ice which is protruded below the snow-line, into the region where the summer thaw exceeds the winter snow-fall, is necessarily subject to continual thinning or wasting from its upper or exposed surface, and thus finally becomes liquefied, and is terminated by direct solar action.

Many of the characteristic phenomena of Alpine glaciers depend upon this; among the more prominent of which are the superficial extrusion of boulders or rock fragments that have been buried in the *nevé* or have fallen into the crevasses of the upper part of the true glacier, and the final deposit of these same boulders or fragments at the foot of the glaciers forming ordinary moraines.

But this is not all. The thawing which extrudes, and finally deposits the larger fragments of rock, sifts from them the smaller particles, the aggregate bulk of which usually exceeds very largely that of the larger fragments. This fine silt or sand thus washed away is carried by the turbid glacier torrent to considerable distances, and deposited as an alluvium wherever the agitated waters find a resting-place.

Thus the *débris* of the ordinary modern glacier is effectively separated into two or more very distinct deposits, the moraine at the glacier foot consisting of rock fragments of considerable size, with very little sand or clay or other fine deposit between them, and a distant deposit of totally different character, consisting of gravel, sand, clay, or mud, according to the length and conditions of its journey. The "chips," as they have been well called, are thus separated from what I may designate the *filings* or *sawdust* of the glacier.

The filings from the existing glaciers of the Bernese Alps are gradually filling up the lake basins of Geneva and Constance, repairing the breaches made by the erosive action of their gigantic predecessors; those of the southern slope of the Alps are doing a large share in filling up the Adriatic; while the chips of all merely rest upon the glacier beds forming the comparatively insignificant terminal moraine deposits.

The same in Scandinavia. The Storelv of the Jostedal is fed by the melting of the Krondal, Nygaard, Bjornestegs, and Soldal glaciers. It has filled up a branch of the deep Sogne fjord, forming an extensive fertile plain at the mouth of its wild valley, and is depositing another subaqueous

plain beyond, while the moraines of the glaciers are but inconsiderable and comparatively insignificant heaps of loose boulders, spread out on the present and former shores of the above-named glaciers, which are overflows from one side of the great *nevé*, the Jostedal Sneefornd. All of these glaciers flow down small lateral valleys, spread out, and disappear in the main valley, which has now no glacier of its own, though it was formerly glaciated throughout.

What must have been the condition of this and the other great Scandinavian valleys when such was the case? To answer this question rationally we must consider the meteorological conditions of that period. Either the climate must have been much colder or the amount of precipitation vastly greater than at present, in order to produce the general glaciation that rounded the mountains up to a height of some thousands of feet above the present sea-level. Probably both factors co-operated to effect this vast glaciation, the climate colder, and the snow-fall also greater. The whole of Scandinavia, or as much as then stood above the sea, must have been a *nevé* or sneefornd on which the annual snow-fall exceeded the annual thaw.

This is the case at present on the largest *nevé* of Europe, the 500 square miles of the great plateau of the Jostedals and Nordfjords Sneefornd; on all the overflowing *nevé* or snow-fields of the Alps above the snow-line, over the greater part of Greenland, and (as the structure of the southern icebergs prove) everywhere within the great Antarctic ice barrier.

What, then, must happen when the snow-line comes down, or nearly down, to the sea-level? It is evident that the outthrust glaciers, the overflow down the valleys, cannot come to an end like the present Swiss and Scandinavian glaciers, by the direct melting action of the sun. They may be somewhat thinned from below by the heat of the earth, and that generated by their own friction on the rocks, but these must be quite inadequate to overcome the perpetual accumulation due to the snow-fall upon their own surface and the vast overflow from the great snow-fields above. They must go on and on, ever increasing, until they meet some new condition of climate or some other powerful agent of dissipation—something that can effectively melt them.

This agent is very near at hand in the case of the Scandinavian valleys and those of Scotland. It is the sea. I think I may safely say that the valley glaciers of these countries during the great ice age *must* have reached the

sea and there terminated their existence, just as the Antarctic glaciers terminate at the present Antarctic ice-wall.

What must happen when a glacier is thus thrust out to sea? This question is usually answered by assuming that it slides along the bottom until it reaches such a depth that flotation commences, and then it breaks off or "calves" as icebergs. This view is strongly expressed by Mr. Geikie (p. 47) when he says that—"The seaward portion of an Arctic glacier cannot by any possibility be floated up without sundering its connection with the frozen mass behind. So long as the bulk of the glacier much exceeds the depth of the sea, the ice will of course rest upon the bed of the fjord or bay without being subjected to any strain or tension. But when the glacier creeps outwards to greater depths, then the superior specific gravity of the sea-water will tend to press the ice upward. That ice, however, is a hard continuous mass, with sufficient cohesion to oppose for a time this pressure, and hence the glacier crawls on to a depth far beyond the point at which, had it been free, it would have risen to the surface and floated. If at this great depth the whole mass of the glacier could be buoyed up without breaking off, it would certainly go to prove that the ice of Arctic regions, unlike ice anywhere else, had the property of yielding to mechanical strain without rupturing. But the great tension to which it is subjected takes effect in the usual way, and the ice yields, not by bending and stretching, but by breaking." Mr. Geikie illustrates this by a diagram showing the "calving" of an iceberg.

In spite of my respect for Mr. Geikie as a geological authority, I have no hesitation in contradicting some of the physical assumptions included in the above.

Ice has no such rigidity as here stated. It does possess in a high degree "the property of yielding to mechanical strain without rupturing." We need not go far for evidence of this. Everybody who has skated or seen others skating on ice that is but just thick enough to "bear" must have felt or seen it yield to the mechanical strain of the skater's weight. Under these conditions it not only bends under him, but it afterwards yields to the reaction of the water below, rising and falling in visible undulations, demonstrating most unequivocally a considerable degree of flexibility. It may be said that in this case the flexibility is due to the thinness of the ice; but this argument is unsound, inasmuch as the manifestation of such flexibility does not depend upon absolute thickness or thinness, but upon the

relation of thickness to superficial extension. If a thin sheet of ice can be bent to a given arc, a thick sheet may be bent in the same degree, but the thicker ice demands a greater radius and proportionate extension of circumference. But we have direct evidence that ice of great thickness—actual glaciers—may bend to a considerable curvature before breaking. This is seen very strikingly when the uncrevassed ice-sheet of a slightly inclined *nevé* suddenly reaches a precipice and is thrust over it. If Mr. Geikie were right, the projecting cornice thus formed should stand straight out, and then, when the transverse strain due to the weight of this rigid overhang exceeded the resistance of tenacity, it should break off short, exposing a face at right angles to the general surface of the supported body of ice. Had Mr. Geikie ever seen and carefully observed such an overhang or cornice of ice, I suspect that the above quoted passage would not have been written.

Some very fine examples of such ice-cornices are visible from the ridge separating the Handspikjen Fjelde from the head of the Jostedal, where a fine view of the great *nevé* or sneefornd is obtained. This side of the *nevé* terminates in precipitous rock-walls; at the foot of one of these is a dreary lake, the Styggevand. The overflow of the *nevé* here forms great bending sheets that reach a short way down, and then break off and drop as small icebergs into the lake.*

The ordinary course of glaciers afford abundant illustrations of the plasticity of such masses of ice. It spreads out where the valley widens, contracts where the valley narrows, and follows all the convexities or concavities of the axial line of its bed. If the bending thus enforced exceeds a certain degree of abruptness crevasses are formed, but a considerable bending occurs before the rupture is effected, and crevasses of considerable magnitude are commonly formed without severing one part of a glacier from another. They are usually V-shaped, in vertical section, and in many the rupture does not reach the bottom of the glacier. Very rarely indeed does a crevass cross the whole breadth of a glacier in such a manner as to completely separate, even temporarily, the lower from the upper part of the glacier.

If a glacier can thus bend *downwards* without "sundering its connection with the frozen mass behind," surely it may bend upwards in a corresponding degree, either with or

* *See* "Through Norway with a Knapsack," chapters 11 and 12, for further descriptions of these.

without the formation of crevasses, according to the thickness of the ice and the degree of curvature.

A glacier reaching the sea by a very steep incline would probably break off, in accordance with Mr. Geikie's description, just as an Alpine glacier is ruptured fairly across when it makes a cascade over a suddenly precipitous bend of its path. One entering the sea at an inclination somewhat less precipitous than the minor limit of the effective rupture gradient it would be crevassed in a contrary manner to the crevassing of Alpine glaciers. Its crevasses would gape downwards instead of upwards—have an A-shaped instead of a V-shaped section.

With a still more moderate slope, the up-floating of the termination of the glacier, and a concurrent general uplifting or upbending of the submerged portion of the glacier might occur without even a partial rupture or crevasse formation occurring.

Let us now follow out some of the necessary results of these conditions of glacier existence or glacial prolongation. The first and most notable, by its contrast with ordinary glaciers, is the absence of lateral, medial, or terminal moraines. The larger masses of *débris*, the chippings that may have fallen from the exposed escarpments of the mountains upon the surface of the upper regions of the glacier, instead of remaining on the surface of the ice and standing above its general level by protecting to some extent the ice on which they rest from the general snow-thaw, would become buried by the upward accretion of the ice due to the unthawed stratum of each year's snow-fall.

The only thinning agency at work upon such glaciers during their journey over the *terra firma* being the outflow of terrestrial heat, and that due to their friction upon their beds, the thinning must all take place from below, and thus, as the glacier proceeds downwards, these rock fragments must be continually approaching the bottom instead of continually approaching the top, as in the case of modern Alpine glaciers flowing below the snow-line.

An important consequence of this must be that the erosive power of these ancient glaciers was, *cæteris paribus*, greater than that of modern Alpine glaciers, especially if we accept those theories which ascribe an actual internal growth or regeneration of glaciers by the relegation below of some of the water resulting from the surface-thaw.

It follows, therefore, that such glaciers could not deposit any moraines such as are in course of deposition by existing Alpine and Scandinavian glaciers.

What, then, must become of the chips and filings of these outfloating glaciers? They must be carried along with the ice *so long as that ice rests upon the land*; for this *débris* must consist partly of fragments imbedded in the ice, and partly of ground and re-ground excessively subdivided particles, forming a slimy mud that must either cake into what I may call ice-mud, and become a part of the glacier, or flow as liquid mud or turbid water beneath it, as with ordinary glaciers. The quantity of water being relatively small under the supposed conditions, the greater part would be carried forward to the sea by the ice rather than by the water.

As the glacier with its lower accumulation advanced into deeper and deeper water, its pressure upon its bed must progressively diminish until it reaches a line where it would just graze the bottom with a touch of feathery lightness. Somewhere before reaching this it would begin to deposit its burden on the sea-bottom, the commencement of this deposition being determined by the depth whereat the tenacity of the deposit, or its friction against the sea-bottom, or both combined, becomes sufficient to overpower the now-diminished pressure and forward thrusting, or erosive power of the glacier.

Farther forward, in deeper water, where the ice becomes fairly raised above the original sea-bottom, a rapid thawing must occur by the action of the sea-water, and if any communication existed between this ice-covered sea and the waters of warmer latitudes a further thawing must result from the currents that would necessarily be formed by the interchange of water of varying specific gravities. Deposition would thus take place in this deeper water, continually shallowing it or bringing up the sea-bottom nearer to the ice-bottom.

This raising of the sea-bottom must occur not only here, but farther back, *i.e.*, from the line at which any deposition commenced. This line or region, whereat the depth is just sufficient to allow the ice to rest lightly on its own deposit and slide over it without either sweeping it forward or depositing any more upon it, becomes an interesting critical region, subject to continuous forward extension during the lifetime of the glacier, as the deposition beyond it must continually raise the sea-bottom until it reaches this critical depth at which this deposition must cease. This would constitute what I may designate the normal depth of the glaciated sea, or the depth to which it would be continually tending, during a great glacial epoch, by the formation of a submarine bank or plain of glacier deposit, over which the

FIG. 1.—LOW GLACIATED ROCKS OF THE THRENEN ISLANDS, NEAR THE COAST.

FIG. 2.—OUTLYING LOFODENS, NOT GLACIATED.

glacier would slide without either grinding it lower by erosion or raising it higher by deposition.

But what must be the nature of this deposit? It is evident that it cannot be a mere moraine consisting only of the larger fragments of rock such as are now deposited at the foot of glaciers that die out before reaching the sea. Neither can it correspond to the glacial silt which is washed away and separated from these larger fragments by glacial streams, and deposited at the outspreadings of glacier torrents and rivers. It will correspond to neither the assorted gravel, sand, or mud of these alluvial deposits, but must be an agglomeration of all the infusible solid matter the glacier is capable of carrying.

It must contain, in heterogeneous admixture, the great boulders, the lesser rock fragments, the gravel chips, the sand, and the slimy mud; and these settling down quietly in the cold, gloomy waters, overshadowed by the great ice-sheet, must form just such an agglomeration as we find in the boulder clay and tills; and lie just in those places where these deposits abound, provided the relative level of land and sea during the glacial epoch were suitable.

I should make one additional remark relative to the composition of this deposit, viz., that under the conditions supposed, the original material detached from the rocks around the upper portions of the glaciers would suffer a far greater degree of attrition at the glacier bottom than it obtains in modern Alpine glaciers, inasmuch as in these it is removed by the glacier torrent when it has attained a certain degree of fineness, while in the greater glaciers of the glacial epoch it would be carried much further in association with the solid ice, and be subjected to more grinding and re-grinding against the bottom. Hence a larger proportion of slimy mud would be formed, capable of finally indurating into stiff clay such as forms the matrix of the till and boulder clay.

The long journey of the bottom *débris* stratum of the glacier, and its final deposition when in a state of neutral equilibrium between its own tendency to repose and the forward thrust of the glacier, would obviously tend to arrange the larger fragments of rock in the manner in which they are found imbedded in the till, *i.e.*, the oblong fragments lying with their longer axes and their best marked striæ in the direction of the motion of the glacier. The "striated pavements" of the till are thus easily explained, as the surface upon which the ice advanced when its depo-

sits had reached the critical or neutral height. Such a pavement would continually extend outwards.

The only sorting of the material likely to occur under these conditions would be that due to the earlier deposition and entanglement of the larger fragments, thus producing a more stony deposit nearer inland, just as Mr. Geikie describes the actual deposits of till where, "generally speaking, the stones are most numerous in the till of hilly districts; while at the lower levels of the country the clayey character of the mass is upon the whole more pronounced." These "hilly districts," upon the supposition of greater submergence, would be the near shore regions, and the lower levels the deeper sea where the glacier floated freely.

The following is Mr. Geikie's description of the distribution of the till (page 13):—"It is in the lower-lying districts of the country where till appears in greatest force. Wide areas of the central counties are covered up with it continuously, to a depth varying from two or three feet up to one hundred feet and more. But as we follow it towards the mountain regions it becomes thinner and more interrupted —the naked rock ever and anon peering through, until at last we find only a few shreds and patches lying here and there in sheltered hollows of the hills. Throughout the Northern Highlands it occurs but rarely, and only in little isolated patches. It is not until we get away from the steep rocky declivities and narrow glens and gorges, and enter upon the broader valleys that open out from the base of the highland mountains to the low-lying districts beyond, that we meet with any considerable deposits of stony clay. The higher districts of the Southern Uplands are almost equally free from any covering of till."

This description is precisely the same as I must have written, had I so far continued my imaginary sketch of the results of ancient glaciation as to picture what must remain after they had all melted away, and the sea had receded sufficiently to expose their submarine deposits.

Throughout the above I have assumed a considerable submergence of the land as compared with the present sea-level on the coasts of Scotland, Scandinavia, &c.

The universality of the terraces in all the Norwegian valleys opening westward proves a submergence of *at least* 600 or 700 feet. When I first visited Norway, in 1856, I accepted the usual description of these as alluvial deposits; was looking for glacial vestiges in the form of moraines, and thus quite failed to observe the true nature of these vast accumulations, which was obvious enough when I

re-examined them in the light of more recent information. Some few are alluvial, but they are exceptional and of minor magnitude. As an example of such alluvial terraces I may mention those near the mouth of the Romsdal, that are well seen from the Aak Hotel, and which a Russian prince, or other soldier merely endowed with military eyes, might easily mistake for artificial earthworks erected for the defence of the valley.

In this case, as in the others where the terraces are alluvial, the valley is a narrow one, occupied by a relatively wide river loaded with recent glacial *débris*. It evidently filled the valley during the period of glacial recession.

The ordinary wider valleys, with a river that has cut a narrow channel through the outspread terrace-flats, display a different formation. Near the mouth of such valleys I have seen cuttings of more than a hundred feet in depth, through an unbroken terrace of most characteristic till, with other terraces rising above it. This is the ordinary constitution of the *lower portions* of most of the Scandinavian terraces.

These terraces are commonly topped with quite a different stratum, which at first I regarded as a subsequent alluvial or esturine deposit, but further examination suggested another explanation of the origin of some portions of this superficial stratum, to which I shall refer hereafter.

These terraces prove a rise of sea or depression of land, during the glacial epoch, to the extent of 600 feet as a *minimum*, while the well-known deposits of Arctic shells at Moel Tryfaen and the accompanying drift have led Prof. Ramsay to estimate "the probable amount of submergence during some part of the glacial period at about 2300 feet."*

It would be out of place here to reproduce the data upon which geologists have based their rather divergent opinions respecting the actual extent of the submergence of the western coast of North Europe. All agree that a great submergence occurred, but differ only as to its extent, their estimates varying between 1000 and 3000 feet.

There is one important consideration that must not be overlooked, viz., that—if my view of the submarine origin of the till be correct—the mere submergence of the land at the glacial period does not measure the difference between the depth of the sea at that and the present time, seeing that the deposits from the glaciers must have shallowed it very materially.

* Lyell, Elements of Geology, p. 159.

It is only after contemplating thoughtfully the present form of the granitic and metamorphic hills of Scandinavia,—hills that are always angular when subjected only to subaërial weathering,—that one can form an adequate conception of the magnitude of this shallowing deposit. The rounding, shaving, grinding, planing, and universal abrasion everywhere displayed appear to me to justify the conclusion that if the sea were now raised to the level of the terraces, *i.e.*, 600 feet higher than at present, the mass of matter abraded from the original Scandinavian mountains, and lying under the sea, would exceed the whole mass of mountain left standing above it.

The first question suggested by reading Mr. Geikie's book was whether the terraces are wholly or partially formed of till, and more especially whether their lower portions are thus composed. This, as already stated, was easily answered by the almost unanimous reply of all the many Norwegian valleys I traversed. Any tourist may verify this. The next question was whether this same till extends below the sea. This was not so easily answered by the means at my disposal, as I travelled hastily round the coast from Stavanger via the North Cape to the frontier of Russian Lapland in ordinary passenger steam packets, which made their stoppages to suit other requirements than mine. Still I was able to land at many stations, and found that wherever there was a gently sloping strand at the mouth of an estuary, or of a valley whose river had already deposited its suspended matter (a common case hereabouts, where so many rivers terminate in long estuaries or open out into bag-shaped lakes near the coast), and where the bottom had not been modified by secondary glaciation, that the receding tide displayed a sea bottom of till, covered with a thin stratum of loose stones and shells. In some cases the till was so bare that it appeared like a stiff mud deposited but yesterday.

At Bodö, an arctic coast station on the north side of the mouth of the Salten fjord (lat. 67° 20′), where the packets make a long halt, is a very characteristic example of this; a deposit of very tough till forming an extensive plain just on the sea level. The tide rises over this, and the waves break upon it, forming a sort of beach by washing away some of the finer material, and leaving the stones behind. The ground being so nearly level, the reach of the tide is very great, and thus a large area is exposed at low tide. Beyond the limit of high tide is an extensive inland plain covered with coarse grass and weeds growing directly upon the surface of the original flat pavement of till.

There is no river at Bodö; the sea is clear, leaves no ap-

preciable deposit, and the degree of denudation of the clayey matrix of the till is very much smaller than might be expected. The limits of high water is plainly shown by a beach of shells and stones, but at low tide the ground over which the sea has receded is a bare and scarcely modified surface of till. I have observed the same at low water at many other Arctic stations. In the Tromsö sund there are shallows at some distance from the shore which are just covered with water at low tide. I landed and waded on these, and found the bottom to consist of till covered with a thin layer of shells, odd fragments of earthenware, and other rubbish thrown overboard from vessels. It is evident that breakers of considerable magnitude are necessary for the loosening of this compact deposit, that it is very slightly, if at all, affected by the mere flow of running water.

I specify these instances as characteristic and easy of verification, as the packets all stop at these stations, but a yachtsman sailing at leisure amidst the glorious coast scenery of the Arctic Ocean might multiply such observations a hundred fold by stopping wherever such strands are indicated in passing. Of these I saw a multitude in places where I was unable to go ashore and examine them.

A further question in this direction suggested itself on the spot, viz., What is the nature of the "*banks*" which constitute the fishing grounds of Norway, Iceland, Newfoundland, &c. They are submarine plains unquestionably—they must have a high degree of fertility in order to supply food for the hundreds of millions of voracious cod-fish, coal-fish, haddocks, hallibut, &c., that people them. These large fishes all *feed on the bottom*, their chief food being mollusca and crustacea, which must find, either directly or indirectly, some pasture of vegetable origin. The banks are, in fact, great meadows or feeding grounds for the lower animals which support the higher.

From the Lofoten bank alone 20 millions of cod-fish are taken annually, besides those devoured by the vast multitude of sea birds. Now this bank is situated precisely where, according to the above-stated view of the origin of the till, there should be a huge deposit. It occupies the Vest fjord, *i.e.*, the opening between the mainland and the Lofoden Islands, extending from Moskenes, to Lodingen on Hindö, just where the culminating masses of the Kjølen Mountains must have poured their greatest glaciers into the sea by a westward course, and these glaciers must have been met by another stream pouring from the north, formed by the glaciers of Hindö, Senjenö, and both must have coalesced

with a third flood pouring through the Ofoten fjord, the Tys fjord, &c., from the mainland. The Vest fjord is about 60 miles wide at its mouth, and narrows northward till it terminates in the Ofoten fjord, which forks into several branches eastward. A glance at a good map will show that here, according to my explanation of the origin of the till, there should be the greatest of all the submarine plains of till which the ancient Scandinavian glaciers have produced, and of which the plains of till I saw on the coast at Bodö (which lies just at the mouth of the Vest fjord, where the Satten fjord flows into it), are but the slightly inclined continuation.

Some idea of this bank may be formed from the fact that outside of the Lofodens the sea is 100 to 200 fathoms in depth, that it suddenly shoals up to 16 or 20 fathoms on the east side of these rocks, and this shallow plain extends across the whole 50 or 60 miles between these islands and the mainland.* It must not be supposed the fjords or inlets of Scandinavia are *usually* shallower than the open sea; the contrary is commonly the case, especially with the narrowest and those which run farthest inland. They are *very much* deeper than the open sea.

If space permitted I could show that the great Storregen bank, opposite Aalesund and Molde, where the Stor fjord, Mold fjord, &c., were the former outlets of the glaciers from the highest of all the Scandinavian mountains, and the several banks of Finmark, &c., from which in the aggregate are taken another 20 or 30 millions of cod-fish annually, are all situated just where theoretically they ought to be found. The same is the case with the great bank of Newfoundland and the banks around Iceland, which are annually visited by large numbers of French fishermen from Dunkerque, Boulogne, and other ports.

Whenever the packet halted over these banks during our coasting trip we demonstrated their fertility by casting a line or two over the bulwark. No bait was required, merely a double

* The celebrated "maelström" is one of the currents that flow down the submarine incline between these islands when the tide is falling. Although I have ridiculed some of the accounts of this now innocent stream, I am not prepared to assert that it was always as mild as at present. If the ancient glaciers were stopped suddenly, as they may well have been, by the rocky barrier of Mosken, between Vaerö and Moskenesö, and they then suddenly concluded their deposition of till, a precipice must have been formed between this and the deep sea outside the islands, down which the sea would pitch when the tide was falling, and thus form some dangerous eddies. This cascade would gradually obliterate itself by wearing down the precipitous wall to an inclined plane such as at present exists, and down which the existing current flows.

hook with a flat shank attached to a heavy leaden plummet. The line was sunk till the lead touched the bottom, a few jerks were given, and then a tug was felt: the line hauled in with a cod-fish or hallibut hooked, not inside the mouth, but externally by the gill-plates, the back, the tail, or otherwise. The mere jerking of a hook near the bottom was sufficient to bring it in contact with some of the population. There is a very prolific bank lying between the North Cape and Nordkyn, where the Porsanger and Laxe fjord unite their openings. Here we were able, with only three lines, to cover the fore-deck of the packet with struggling victims in the course of short halts of fifteen to thirty minutes. Not having any sounding apparatus by which to fairly test the nature of the sea-bottom in these places, I cannot offer any direct proof that it was composed of till. By dropping the lead I could *feel* it sufficiently to be certain that it was not rock in any case, but a soft deposit, and the marks upon the bottom of the lead, so far as they went, afforded evidence in favour of its clayey character. A further investigation of this would be very interesting.*

But the most striking—I may say astounding—evidence of the fertility of these banks, one which appeals most powerfully to the senses, is the marvellous colony of sea-birds at Sverholtklubben, the headland between the two last-named fjords. I dare not estimate the numbers that rose from the rocks and darkened the sky when we blowed the steam-whistle in passing. I doubt whether there is any other spot in the world where an equal amount of animal life is permanently concentrated. All these feed on fish, and an examination of the map will show why—in accordance with the above speculations—they should have chosen Sverholtklubben as the best fishing ground on the arctic face of Europe.

I am fully conscious of the main difficulty that stands in the way of my explanation of the formation of the till, viz., that of finding sufficient water to float the ice, and should have given it up had I accepted Mr. Geikie's estimate of the thickness of the great ice sheet of the great ice age.

He says (page 186) that "The ice which covered the low grounds of Scotland during the early cold stages of the glacial epoch was certainly more than 2000 feet in thickness, and it must have been even deeper than this between the mainland and the Outer Hebrides. To cause such a mass to float, the

* For further particulars concerning this kind of fishing see "Through Norway with Ladies," which will be ready at about the same time as this article appears.

sea around Scotland would require to become deeper than now by 1400 or 1500 feet at least."

I am unable to understand by what means Mr. Geikie measured this depth of the ice which covered these low grounds, except by assuming that its surface was level with that of the upper ice marks of the hills beyond. The following passage on page 63 seems to indicate he really has measured it thus:—

"Now the scratches may be traced from the islands and the coast-line up to an elevation of at least 3500 feet; so that ice must have covered the country to that height at least. In the Highlands the tide of ice streamed out from the central elevations down all the main straths and glens; and by measuring the height attained by the smoothed and rounded rocks we are enabled to estimate roughly the probable thickness of the old ice-sheet. But it can only be a rough estimate, for so long a time has elapsed since the ice disappeared, the rain and frost together have so split up and worn down the rocks of these highland mountains, that much of the smoothing and polishing has vanished. But although the finer marks of the ice chisel have thus frequently been obliterated, yet the broader effects remain conspicuous enough. From an extensive examination of these we gather that the ice could not have been less, and was probably more, than 3000 feet thick in its deepest parts."

Page 80 he says—"Bearing in mind the vast thickness reached by the Scotch ice-sheet, it becomes very evident that the ice would flow along the bottom of the sea with as much ease as it poured across the land, and every island would be surmounted and crushed, and scored and polished just as readily as the hills of the mainland were."

Mr. Geikie describes the Scandinavian ice-sheet in similar terms, but ascribes to it a still greater thickness. He says (page 404)—"The whole country has been moulded and rubbed and polished by an immense sheet of ice, which could hardly have been less than 6000 or even 7000 feet thick," and he maintains that this spread over the sea and coalesced with the ice-sheet of Scotland.

My recollection of the Lofoden Islands, which from their position afford an excellent crucial test of this question, led me to believe that their configuration presented a direct refutation of Mr. Geikie's remarkable inference; but a mere recollection of scenery being too vague, a second visit was especially desirable in reference to this point. The result of the special observations I made during this second visit fully confirmed the impression derived from memory.

I found in the first place that all along the coast from Stavanger to the Varanger fjord every rock *near the shore* is glaciated; among the thousands of low-lying ridges that peer above the water to various heights none near the mainland are angular. The general character of these is shown in the sketch of "My Sea Serpent," in the last edition of "Through Norway with a Knapsack."

The rocks which constitute the extreme outlying limits of the Lofoden group, and which are between 60 and 70 miles from the shore, although mineralogically corresponding with those near the shore, are totally different in their conformation, as the sketch of three characteristic specimens plainly shows. Mr. Everest very aptly compares them to shark's teeth. Proceeding northward, these rocks gradually progress in magnitude, until they become mountains of 3000 to 4000 feet in height; their outspread bases form large islands, and the west fjord gradually narrows.

The remarkably angular and jagged character of these rocks when weathered in the air renders it very easy to trace the limits of glaciation on viewing them at a distance. The outermost and smallest rocks show from a distance no signs of glaciation. If submerged, the ice of the great ice age must have floated over them; if above the sea as at present, they can have suffered no more glaciation than would be produced by such an ice-sheet as that of the "paleocrystic" ice recently found by Capt. Nares on the North of Greenland. Progressing northward, the glaciation begins to become visible, running first up to about 100 feet above the sea-level on the islands lying westward and southward of Ost Vaagen. Further northward along the coast of Ost Vaagen and Hindö, the level gradually rises to about 500 feet on the northern portion of Ost Vaagen, and up to more than 1000 feet on Hindö, while on the mainland it reaches 3000 to 4000 feet.

A remarkable case of such variation, or descent of ice-level as the ice-sheet proceeded seaward is shown at Tromsö. This small oblong island (lat. 69° 40'), on which is the capital town of Finmark, lies between the mainland and the large mountainous island of Kvalö, with a long sea-channel on each side, the Tromösund and the Sandesund; the total width of these two channels and the island itself being about 4 or 5 miles. The general line of glaciation from the mainland crosses the channels and the island, which has evidently been buried and ground down to its present moderate height of two or three hundred feet. Both of the channels are till-paved. On the east or inland side the mountains near

the coast are glaciated to their summits—are simply *roches-montonnes* over which the reindeer of the Tromsdal Lapps range and feed. On the west the mountains are dark pyramidal peaks, with long vertical snow streaks marking their angular masses.

The contrast is very striking when seen from the highest part of the island, and is clearly due to a decline in the thickness of the ice-sheet in the course of its journey across this narrow channel. Speaking roughly from eye estimation, I should say that this thinning or lowering of the limits of glaciation exceeds 500 feet between the opposite sides of the channel, which, allowing for the hill slopes, is a distance of about 6 miles. This very small inclination would bring a glacier of 3000 feet in thickness on the shore down to the sea-level in an outward course of 30 miles, or about half the distance between the mainland and the outer rocks of the Lofodens.

I am quite at a loss to understand the reasoning upon which Mr. Geikie bases his firm conviction respecting the depth of the ice sheet on the low grounds of Scotland and Scandinavia. He seems to assume that the glaciers of the great ice age had little or no superficial down slope corresponding to the inclination of the base on which they rested. I have considerable hesitation in attributing this assumption to Mr. Geikie, and would rather suppose that I have misunderstood him, as it is a conclusion so completely refuted by all we know of glacier phenomena and the physical laws concerned in their production, but the passages I have quoted and several others are explicit and decided.

Those geologists who contend for the former existence of a great polar ice-cap radiating outwards and spreading into the temperate zones might adopt this mode of measuring its thickness, but Mr. Geikie rejects this hypothesis, and shows by his map of "The Principal Lines of Glacial Erosion in Sweden, Norway, and Finland," that the glaciation of the extreme north of Europe proceeded from south to north; that the ice was formed on land, and proceeded seawards in all directions.

I may add to this testimony that presented by the North Cape, Sverholt, Nordkyn, and the rest of the magnificent precipitous headlands that constitute the characteristic feature of the arctic face of Europe. They stand forth defiantly as a phalanx of giant heralds proclaiming aloud the fallacy of this idea of southward glacial radiation; and in concurrence with the structure and striation of the great glacier troughs that lie between them, and the planed table-land at

their summits, they establish the fact that during the greatest glaciation of the glacial epoch the ice-streams were formed on land and flowed out to sea, just as they now do at Greenland, or other parts of the world where the snow-line touches or nearly approaches the level of the sea.

All such streams must have followed the slope of the hill-sides upon which they rested and down which they flowed, and thus the upper limits of glaciation afford no measure whatever of the thickness of the ice upon "the low grounds of Scotland," or of any other glaciated country. As an example I may refer to Mont Blanc. In climbing this mountain the journey from the lower ice-wall of the Glacier de Bossons up to the *bergschrund* above the *Grand plateau* is over one continuous ice-field, the level of the upper part of which is about 10,000 feet above its terminal ice-wall. Thus, if we take the height of the striations or smoothings of the upper *nevé*, above the low grounds on which the ice-sheet rests, and adopt Mr. Geikie's reasoning, the lower ice-wall of the Glacier de Bossons should be 10,000 feet thick. Its actual thickness, as nearly as I can remember, is about 10 or 12 feet.

Every other known glacier presents the same testimony. The drawing of a Greenland glacier opposite page 47 of Mr. Geikie's book shows the same under arctic conditions, and where the ice-wall terminates in the sea.

I have not visited the Hebrides, but the curious analogy of their position to that of the Lofodens suggests the desirability of similar observations to those I have made in the latter. If the ice between the mainland and the Outer Hebrides was, as Mr. Geikie maintains, "certainly more than 2000 feet in thickness," and this stretched across to Ireland, besides uniting with the still thicker ice-sheet of Scandinavia, these islands should all be glaciated, especially the smaller rocks. If I am right the smaller outlying islands, those south of Barra, should, like the corresponding rocks of the Lofodens, display no evidence of having been overswept by a deep "*mer de glace*."

I admit the probability of an ice-sheet extending as Mr. Geikie describes, but maintain that it thinned out rapidly seaward, and there became a mere ice-floe, such as now impedes the navigation of Smith's Sound and other portions of the Arctic Ocean. The Orkneys and Shetland, with which I am also unacquainted, must afford similar crucial instances, always taking into account the fact that the larger islands may have been independently glaciated by the accumulations due to their own glacial resources. It is the small rocks standing at considerable distance from the shores

of larger masses of land that supply the required test-conditions.

From the above it will be seen that I agree with Mr. Geikie in regarding the till as a "*moraine profonde*," but differ as to the mode and place of its deposition. He argues that it was formed under glaciers of the thickness he describes, while their whole weight rested upon it.

This appears to me to be physically impossible. If such glaciers are capable of eroding solid rocks, the slimy mud of their own deposits could not possibly have resisted them. The only case where this might have happened is where a mountain-wall has blocked the further downward progress of a glacier, or in pockets or steep hollows which a glacier might have bridged over and filled up; but such pockets are by no means the characteristic localities of till, though the till of Switzerland may possibly show examples of the first case. The great depth of the inland lakes of Norway, their bottoms being usually far below that of the present sea-bottom, is in direct contradiction of this.* They should, before all places, be filled with till, if the till were a ground moraine formed on land; but all we know of them confirms the belief that the glaciers deepened them by erosion instead of shallowing them by deposition.

Mr. Geikie's able defence of Ramsay's theory of lake basin erosion is curiously inconsistent with his arguments in favour of the ground moraine.

I fully concur with Mr. Geikie's arguments against the iceberg theory of the formation of the till. This I think he has completely refuted.

Before concluding I must say a few words on those curious lenticular beds of sand and gravel in the till which appear so very puzzling. A simple explanation is suggested in connection with the above-sketched view of the formation of the till. All glaciers, whether in arctic or temperate climates, are washed by streamlets during summer, and these commonly terminate in the form of a stream or cascade pouring down a "*moulin*," or well bored by themselves and reaching the bottom of the glacier. Now what must be the action of such a downflow by water upon my supposed submarine bed

* The largest of the Norwegian lakes, the Mjosen, is 1550 feet deep, and its surface 385 feet above the sea level. Its bottom is about 1000 feet lower than the sea outside, or 500 to 800 feet below the bottom of the Christania Fjord. The fjords, generally speaking, are very much shallower near their mouths than further inland, as though their depth had been determined by the thickness of the glaciers flowing down them, and the consequent limits of flotation and deposition.

of till just grazing the bottom of the glacier? Obviously to wash away the fine clayey particles, and leave behind the coarser sand or gravel. It must form just such a basin or lenticular cavity as Mr. Geikie describes. The oblong shape of these, their longer axis coinciding with the general course of the glacier, would be produced by the onward progress of the moulin. The accordance of their other features with this explanation will be seen on reading Mr. Geikie's description (pp. 18, 19, &c.).

The general absence of marine animals and their occasional exceptional occurrence in the intercalated beds is just what might be expected under the conditions I have sketched. In the gloomy subglacial depths of the sea, drenched with continual supplies of fresh water and cooled below the freezing-point by the action of salt water on the ice, ordinary marine life would be impossible; while, on the other hand, any recession of the glacial limit would restore the conditions of arctic animal life, to be again obliterated with the renewed outward growth of the floating skirts of the inland ice mantle.

But I must now refrain from the further discussion of these and other collateral details, but hope to return to them in another paper.

In "Through Norway with Ladies" I have touched lightly upon some of these, and have more particularly described some curious and very extensive evidences of secondary glaciation that quite escaped my attention on my first visit, and which, too, have been equally overlooked by other observers. In the above I have endeavoured to keep as nearly as possible to the main subject of the origin of the till and the character of the ancient ice-sheet.

VI. THE UNITED STATES GEOLOGICAL AND GEOGRAPHICAL SURVEY OF THE TERRITORIES.—EXPLORATIONS IN 1876.

By Prof. F. V. HAYDEN, Geologist in Charge.

THE various parties composing the United States Geological and Geographical Survey of the Territories commenced their field-work in August. Owing to the evidences of hostility among the northern tribes of Indians it was deemed most prudent to confine the labours of the Survey to the completion of the Atlas of Colorado. Therefore the work of the season of 1876 was a continuation of the labours of the three preceding years, westward, finishing the entire mountainous portion of Colorado, with a belt of 15 miles in width of northern New Mexico, and a belt 25 miles in breadth of eastern Utah. Six sheets of the Physical Atlas are now nearly ready to be issued from the press. Each sheet embraces an area of over 11,500 square miles, or a total of 70,000 square miles. The maps are constructed on a scale of 4 miles to 1 inch, with contours of 200 feet, which will form the basis on which will be represented the geology, mines, grass, and timber lands, and all lands that can be rendered available for agriculture by irrigation. The areas of exploration the past season are located in the interior of the continent, far remote from settlements, and among the hostile bands of Ute Indians that attacked two of the parties the previous year.

The point of departure the past season was Cheyenne, Wyoming Territory. Two of the parties, with all their outfit, were transported by railroad to Rawlins Springs, and proceeded thence southward. The other two were sent by railroad from Cheyenne southward, one party to Trinidad and the other to Cañon City.

The primary triangulation party was placed in charge of A. D. Wilson, and took the field from Trinidad, the southern terminus of the Denver and Rio Grande Railroad, August 18th, making the first station on Fisher's Peak. From this point the party marched by the valley of the Purgatoire,

crossed the Sangre de Cristo range by way of Costilla Pass, followed the west base of the range northward as far as Fort Garland, making a station on Culebra Peak.

About 6 miles north of Fort Garland is located one of the highest and most rugged mountain peaks in the west, called Blanca Peak, the principal summit of the Sierra Blanca group. On the morning of August 28th the party, with a pack-mule to transport the large theodolite, followed up a long spur which juts out to the south. They found no difficulty in riding to timber line, which is here about 12,000 feet above sea level. At this point they were compelled to leave the animals, and, distributing the instruments among the different members of the party, proceeded on foot up the loose, rocky slope to the first outstanding point, from which a view could be obtained of the main peak of the range. Although this first point is only 600 feet lower than the main summit, yet the most arduous portion of the task was to come. The main summit is about 2 miles north of the first point, in a straight line, and connected with it by a very sharp-toothed, zigzag ridge, over which it is most difficult to travel, on account of the very loose rocks and the constant fear of being precipitated down on either side several hundred feet into the amphitheatres below. After some two hours of this difficult climbing they came to the base of the main point, which, though very steep, was soon ascended, and at 11 o'clock A.M. they found themselves on the very summit. From this point one of the most magnificent views in all Colorado was spread out before them. The greater portion of Colorado and New Mexico was embraced in this field of vision. This point is the highest in the Sierra Blanca group, and, so far as is known at the present time, is the highest in Colorado. The elevation of this point was determined by Mr. Wilson in the following manner:—First, by a mean of eight barometric readings, taken synchronously with those at Fort Garland, which gave a difference between the two points of 6466 feet; secondly, by fore and back angles of elevation and depression, which gave a difference of 6468 feet. The elevation at the fort was determined by a series of barometric readings, which, when compared with the Signal Service barometer at Colorado Springs, gave it an elevation of 7997 feet, making the Blanca Peak 14,464 feet above the sea level. This peak may be regarded, therefore, as the highest, or at least next to the highest, yet known in the United States. A comparison with some of the first-class peaks in Colorado will show the relative height:—

			Feet.
Uncompahgre Peak,	above sea level	. .	14,235
Blanca Peak,	,, ,,	. .	14,464
Mount Harvard,	,, ,,	. .	14,384
Gray's Peak,	,, ,,	. .	14,341
Mount Lincoln,	,, ,,	. .	14,296
Mount Wilson,	,, ,,	. .	14,280
Long's Peak,	,, ,,	. .	14,271
Pike's Peak	,, ,,	. .	14,146

The foregoing table will afford some conception of the difficulty encountered in determining the highest peak where there are so many that are nearly of the same elevation. About fifty peaks are found within the limits of Colorado that exceed 14,000 feet above the sea level.

From this point the party proceeded westward across the San Luis Valley, and up the Rio Grand to its source, making two primary stations on the way, one near the summit district and the other on the Rio Grande pyramid. From the head of the Rio Grande the party crossed the continental divide, striking the Animas Park, and thence proceeded by trail to Parrott City.

After making a station on La Plata Peak the party marched north-west across the broken mesa country west of the Dolores, making three stations on the route to complete a small piece of topography that had been omitted the previous year, on account of the hostility of the Ute Indians. After making a primary station on the highest point of the Abajo mountains, the party turned eastward to Lone Cone, where another station was made. Thence crossing the Gunnison and Grand Rivers, they proceeded to the great volcanic plateau at the head of White River. The final station was made between the White and Yampah Rivers, in the north-western corner of Colorado. During this brief season Mr. Wilson finished about 1000 square miles of topography, and made eleven primary geodetic stations, thus connecting together by a system of primary triangles the whole of Southern and Western Colorado.

In company with the triangulation party, Mr. Holmes made a hurried trip through Colorado, touching, also, portions of New Mexico and Utah. He was unable to pay much attention to detailed work, but had an excellent opportunity of taking a general view of the two great plain-belts that lie the one along the east, the other along the west base of the Rocky Mountains. For nearly 2000 miles' travel he had constantly in view the cretaceous and tertiary formations among which are involved some of the most interesting

geological questions. He observed, among other things, the great persistency of the various groups of rocks throughout the east, west, and north, and especially in the west; that from northern New Mexico to south-west Wyoming, the various members of the cretaceous lie in almost unbroken belts.

Between the east and west there is only one great incongruity. Along the east base of the mountains the upper cretaceous rocks, including Nos. 4 and 5, are almost wanting, consisting at most of a few hundred feet of shales and laminated sandstones. Along the west base this group becomes a prominent and important topographical as well as geological feature. In the south-west, where it forms the "Mesa Verde" and the cap of the Dolores plateau, it comprises upward of 2000 feet of coal-bearing strata, chiefly sandstone, while in the north it reaches a thickness of 3500 feet, and forms the gigantic "hog-back" of the Grand River Valley.

While in the south-west he visited the Sierra Abajo, a small group of mountains which lie in eastern Utah, and found, as he had previously surmised, that the structure was identical with that of the four other isolated groups that lie in the same region. A mass of trachyte has been forced up through fissures in the sedimentary rocks, and now rests chiefly upon the sandstones and shales of the lower cretaceous. There is a considerable amount of arching of the sedimentary rocks, caused probably by the intrusion of wedge-like sheets of trachyte, while the broken edges of the beds are frequently but abruptly up, as if by the upward or lateral pressure of the rising mass. He was able to make many additional observations on the geology of the San Juan region, and secured much valuable material for the colouring of the final map.

He states that the northern limit of ancient cliff-builders in Colorado and Eastern Utah is hardly above latitude 37° 45′.

The Grand River division was directed by Henry Gannett, topographer, with Dr. A. C. Peale as geologist. James Stevenson, executive officer of the Survey, accompanied this division, for the purpose of assisting in the management of the Indians, who last year prevented the completion of the work in their locality by their hostility.

The work assigned this division consisted in part of a small area, containing about 1000 square miles, lying south of the Sierra la Sal. The greater portion of the work of this division lay north of the Grand River, limited on the

north by the parallel of 39° 30′, and included between the meridian of 108° and 109° 30′.

This division took the field at Canon City, Colorado, about the middle of August. The party travelled nearly west up the Arkansas River, over Marshall's Pass, and down the Tomichi and Gunnison Rivers to the Uncompahgra (Ute) Indian agency. Here they secured the services of several Indians as escort to the somewhat dangerous country which they were first to survey. This area, lying south of Sierra la Sal, was worked without difficulty. It is a broken plateau country, and presents many extremely curious pieces of topography. Eleven days were occupied in this work.

The Grand River, from the mouth of the Gunnison River to that of the Dolores, *i.e.*, for nearly a hundred miles, flows along the southern edge of a broad valley, much of the way being in a low canon, 100 to 200 feet deep. The course of the river is first north-west for 25 miles; then turning abruptly, it flows south-west, and then south, for about 75 miles. This valley has an average width of 12 miles. It is limited on the north and west by the "Roan or Book Cliffs," and their foot-hills, which follow the general course of the river. These cliffs rise from the valley in a succession of steps to a height of about 4000 feet above it, or 8000 to 8500 feet above the sea.

From its crest this plateau (for the Book Cliffs are but the southern escarpment of a plateau) slopes to the N.N.E. at an angle of not more than five degrees. It extends from the Wahsatch mountains, on the west, to the foot hills of the Park range, on the east, and presents everywhere the same characteristics. The Green River crosses it, flowing in a direction exactly the reverse of the dip. It borders the Grand on the north for 100 miles, the crest forming the divide between the Grand and the White. On the south side of the crest are broken cliffs; on the north side, the branches of the White Canon immediately. This leaves the divide in many places very narrow, in some cases not more than 30 to 40 feet wide, with a vertical descent on the south toward the Grand River, and an extremely steep earth-slope (35 degrees in many cases) at the heads of the streams flowing north to the White River. This crest, though not over 8500 feet in height, is the highest land for a long distance in every direction.

After leaving the Uncompahgre agency, the party followed Gunnison's Salt Lake road to the Grand, and down that river to the mouth of the Dolores, in latitude 38° 50′, longitude 109° 17′. At this point they turned northward,

and went up to the crest of the Book plateau. They followed the crest to the eastward for upward of a hundred miles, or to longitude 108° 15′; then descended to the Grand, and followed it up to longitude 107° 35′, and thence *via* the White River (Ute) Indian agency, to Rawlins, where they arrived on October 23rd.

The whole area worked is about 3500 square miles, in surveying which about sixty stations were made.

The geological work of this division, by Dr. Peale, connects directly with that done by him in 1874 and 1875. Sedimentary formations prevail on both districts visited during the past season.

The country first examined lies between the San Miguel and Dolores Rivers, extending northward and north-westward from Lone Cone Mountain. The general character of this region is that of a plateau cut by deep gorges or canons, some of which, especially toward the north, extend from the sandstones of the Dakota group to the top of the Red Beds. The depth of the canon, however, is no indication of its importance as a stream-bed, for, excepting the main streams, they are dry the greater portion of the year. There are not great disturbances of the strata, what folds do occur being broad and comparatively gentle.

The San Miguel River, on leaving the San Juan mountains, flows toward the north-west, and, with its tributaries, cuts through the sandstones of the Dakota group, exposing the variegated beds lying beneath, that have generally been referred to the Jurassic. About 25 or 30 miles north of Lone Cone, the river turns abruptly to the west, and flows west and south-west for about 15 miles, when it again turns and flows generally north-west, until it joins the Dolores. Between the San Miguel and Lone Cone the sandstones of the Dakota group, or No. 1 cretaceous, are nearly horizontal, forming a plateau which, on approaching the mountains, has a copping of cretaceous shales.

Beyond the bend the San Miguel flows in a monoclinal valley, in which the canon walls are of the same description as in the upper part of its course. As the mouth is approached the Red Beds appear. Between this portion of the course of the San Miguel and the almost parallel course of the Dolores, which is in a similar monoclinal rift, there are two anticlinal and two synclinal valleys parallel to each other. They are all occupied by branches of the Dolores lower cretaceous, jurassic, and triassic strata outcrop, and present some interesting geological details, which will be fully considered in the report on the District. The Dolores

River comes from a high plateau in a zigzag course, flowing sometimes with the strike, and sometimes with the dip of the strata. Its general course on the western line is about north-west, from which it turns to the northward and westward, finally changing to north-west again, to its junction with the Grand. It is in canon the greater part of its course.

In the region of country north of Grand River the geological formations extend uninterruptedly from the Red Beds exposed on Grand River to the white tertiary cliffs forming the summit of the "Roan Mountains," or Book Cliffs. The Grand is generally in a canon in the Red Beds; on the north side the No. 1 cretaceous sandstone forms a hog-back, sloping towards the cliffs. Between the crest of this hog-back and the cliffs there is a broad valley formed by the erosion of the soft cretaceous shales which extend to the base of the cliffs, and in some places form their lower portion. The cliffs are composed mainly of cretaceous beds, rising one above another in steps until an elevation of about 8000 feet is reached. The summit is the edge of a plateau sloping to N.N.E. This plateau is cut by the drainage flowing into the White River from the south. These streams rarely cut through the tertiary series.

Coal of poor quality is found in the sandstones of the Dakota group, and also in the sandstones above the middle cretaceous beds. Wherever noticed it was in their seams, and of little economic importance.

The White River division was directed by G. B. Chittenden, as topographer, accompanied by F. M. Endlich, as geologist.

The district assigned to this party as their field for exploration during the season of 1876 commenced from the eastward at longitude 107° 30′, joining on to the work previously done, and extended westward 30 miles into Utah Territory. Its southern boundary was N. latitude 39° 38′, while the White River formed the northern limit. In order to complete to the greatest possible advantage in the short time that could be allowed, it was determined to make the White River Agency head-quarters, and in two trips from there complete the work. About 3800 square miles comprised the area surveyed.

In working up the topography of this district the party spent forty-eight days of absolute field-work, made forty-one main topographical stations and sixteen auxiliary ones, and travelled within the district about 1000 miles. The party ascertained the courses of all the main trails, the location

and quality of almost all the water, which is scanty throughout, and can map with considerable accuracy the topographical forms and all the water-courses. The area is almost entirely devoid of topographical "points," and the topographer is obliged to depend to a considerable degree on those far to the north and south for the triangulation. The country has heretofore been almost entirely unexplored, and was described by the nearest settlers as a broken canon country, extremely dry. It was marked on the maps as a high, undulating plateau, with fresh-water lakes and timber. The party saw no lakes of more than 400 yards in diameter, and only two or three of these. The country is nearly all inhabitable, both winter and summer, and considerable portions of it valuable; and though three-quarters of it is within the Ute Indian reservation, the advantage of a more accurate knowledge of its character can readily be seen.

While working in the low broken country of south-western Colorado last year, Mr. Chittenden made use of a light portable plane-table, and found it of great value. It appeared at that time that its value was greatest in that class of country, and that in a low rolling district, with few prominent points, or in a high mountain country, it would probably be of little or no use. Altitudes were determined by the mercurial barometer, with a base at the White River Indian Agency, and checked by a continuous system of vertical angles. The altitude of the agency has been determined by a series of barometric observations extending over two years and a half, and referred to railroad levels, and can probably be depended on to within a few feet. The altitude of the agency being about 6500 feet, and the altitudes in the district ranging from 5000 to 8000 feet, makes its location the best possible in height for a barometric survey of the region.

It is the intention of the Survey, during the coming year, to publish some tabulated results of the barometric work in Colorado, showing the system, and its accuracy and reliability. This may be of use in future work, since the topography of the whole west must greatly depend on barometric determinations of altitude, and Colorado has furnished almost every possible phase of western topography.

The longest dimension of the work lying east and west, and the White and Grand Rivers running in approximately parallel courses, the district stretched from the White River up over the divide between the Grand and White, and embraced the heads of the lateral drainage of the former river.

The general topography is a gentle rise from the White River toward the south, and a sudden breaking off when

the divide is reached into rugged and often impassable cliffs, known on the maps as the Roan or Book Mountains. The gentle plateau slope of the White River side is cut by almost numberless and often deep canons, and in many cases the surface of the country has been eroded away, leaving broken and most picturesque forms, the lower benches generally covered with cedars and piñons, and the upper rich in grass.

There are four main streams draining into the White River within the limits of our work—a distance of something over 100 miles. The easternmost is a large running stream; the second, though tolerably good water may be found in pools in its bed, carries in the summer no running water for the greater part of its course; the third has for most of its length a trickling stream of the bitterest of alkali water, while the fourth and westernmost one is perfectly dry for some 25 miles from its mouth, and then forks, one branch containing pure sweet water in pools, the other a running stream of bitter alkali. All of these streams have more or less good water at their heads. They travelled nearly the whole length of all these water-courses, but found good trails only in the two middle ones. Trails, which traverse the whole district in every possible direction, keep mostly on the summits of the ridges and plateaus, and by taking care not to cross the canons, the country is very easily travelled through.

The country is almost entirely destitute of timber, and has but little good water. It is, however, abundantly, richly supplied with grass, and, especially in the winter season, must be well stocked with game. It seems well adapted to its present use as an Indian reservation, and is likely to remain for years to come more valuable for them than it could be for settlement.

In the far western portion, and outside the limits of the reservation, one large vein of asphaltum and several small veins were found, and also running springs of the same material, all of which, if once reached by railroads, will prove of great commercial value. These deposits have been spoken of before, but their location has not been accurately determined. The principal vein seen by this party is at present about 100 miles from railroad communication, but less than half that distance from white settlement, and is likely in the present rapid growth of that country to be within a few years made available.

According to the report of F. M. Endlich, the geology of this district is very simple, though interesting. Inasmuch

as but one divide of importance occurred within the district, the work was somewhat simplified. This was formed by the Book Cliffs, between the drainages of the Grand on the south and the White on the north. Both these rivers flow a little south of west, into Green River, which they join in Utah. From the junction of the Grand and Green downward the river is called the Great Colorado. Orograpically, the region surveyed is comparatively simple. The Book Cliffs are the summit of a plateau about 8000 feet above sea-level, continuing unbroken over to the Green River. Toward the south these cliffs fall off very steeply, forming deep canons that contain tributaries of the Grand River. On the north side, with the dip of the strata, the slope is more gentle, although, in consequence of erosion, numerous precipitous cliffs are found. Descending in that direction, the character of the country changes. Instead of an unbroken slope, we find that the plateau has been cut parallel by the White River drainage, and the long characteristic mesas of that region testify to the action of erosion. Approaching the river, constantly descending with the slight dip of the strata, the bluffs become lower and lower. Though the creek-valleys are wide, and at certain seasons no doubt well watered, the vegetation is that of an arid country. Dwarf pines, piñons, and sagebrush abound, to the almost entire exclusion of other trees or grass. Travelling down White River, this character is again found to change. A new series of bluffs, occasioned by heavy superincumbent strata, gives rise to the formation of deep canons. For 45 miles the party followed the canon of the White; that, no doubt, is analogous to that of the Green, and probably closely resembles that of the Colorado in its detail features. Vertical walls enclose the narrow river-bottoms, and the slopes of the higher portions are ornamented by thousands of curiously eroded rocks. "Monuments" of all kinds, and figures that can readily be compared to those of animated beings, enliven the scenery, which otherwise would be very monotonous; 2000 to 3000 feet may be stated as the height of the walls enclosing the White River.

Geologically speaking, the district was one of singular uniformity. Travelling westward, the older formations, reaching back as far as the triassic, were found. This was followed by cretaceous, which in turn was covered by tertiary. About three-quarters of the region surveyed was found to contain beds belonging to this period. Owing to the lithological character of the strata water was a rare luxury in this region, and men and animals were frequently

dependent upon looking for springs. Farther west still the Green River group sets in, forming those numerous canons of which that of the White River is one.

Having completed their work by October 14th, the party marched eastward through Middle Park, and after twelve days of rain and snow reached Boulder City, Colorado.

The field work of the Yampah division during the past season was principally confined to a district of north-eastern Colorado lying between the Yampah and White Rivers, and between Green River and the subordinate range of mountains that lies west of and parallel with the Park range. The area is embraced between parallels 39° 30′ and 40° 30′, and meridian 107° 30′ and 109° 30′.

The party consisted of Mr. G. R. Bechler, topographer, directing, accompanied by Dr. C. A. White, the well known geologist. They proceeded southward from Rawlins Springs, a station on the Union Pacific Railroad, August 6th, toward their field of labour. From Rawlins Springs to Snake River, a distance of 80 miles, table lands form the chief feature of the topography, while from Snake River to the Yampah River the surface is more undulating, and thickly covered with sage. Between the Yampah and White Rivers, a distance of 50 miles, the country is mountainous, and on the divide between the Yampah and White Rivers the elevation is 8000 to 9000 feet. Mr. Bechler, after having formed the geodetic connection with the work of previous years, concluded to finish the more mountainous portion of the area assigned to him, which began from a line of meridian with the White River Agency, and extended westward to about 108° 10′. Here the party found water and grass in abundance, with one exception. The plateau country, however, was so destitute of water, and so cut up with dry gorges or canons, with scarcely any grass or timber of any kind, that travelling was rendered very difficult. The party therefore made White River its base of supply for water and grass, making side trips into the barren hill-tops or plateaus in every direction.

From the Ute Agency, which is located approximately in latitude 38° 58′ and longitude 107° 48′, the White River takes an almost due west course for 15 or 18 miles, most of the way through an open valley, with here and there narrow gorges. About 50 miles from the Agency the river opens into a broad barren valley, with only here and there scanty patches of vegetation. Soon after, the river enters a deep canon, with vertical walls 1000 feet or more in height, and continues to increase in depth until the river flows into the Colorado of the west.

The Yampah, or Bear River, deviates from a westerly course only for a few miles occasionally. Like White River it flows through a plateau country, which rises gently from the river, back for a distance of about eight miles. South of the river lie the Williams River Mountains, which have a gradual slope to the north. Williams fork, flowing from a south-eastern direction, joins the Yampah river west of the junction. The Yampah traverses the country more or less in a canon, occasionally emerging into an open grassy valley, then enters a deep canon, cuts through the Yampah Mountains, when it joins with the Snake River. The place of junction resembles a fine park, surrounded on all sides with eroded terraces and plateau spurs that rise by steps to the divide on either side. This park is about 8 miles in length from east to west. After leaving this park the river enters a hugh fissure in the mountains, where it remains, until, completing its zigzag course, it joins the Green River in longitude 109° 40′, and latitude 32°. After the junction with the Yampah, the Green River continues in a canon for 14 miles, where it passes through the picturesque palisades of Split Mountain into an open broad valley, longitude 109° 15′, latitude 40° 28′, from which point it takes a south-west direction through the Wamsitta Valley, where it unites with the White River. Into both White and Yampah Rivers numerous branches extend from either side, forming deep canons the greater portion of their length. We may say, in brief, that the sides of the valleys expand and contract, at one time forming the beautiful grassy valleys which in olden times were celebrated as the favourite wintering places for the trappers, or contracting so as to form narrow canons or gorges with walls of varied height.

The walls of Yampah canon average about 1000 feet, while the mountains, receding back to the northward, attain an elevation of 4200 feet, while the highest point of the plateau on the south side is 3400 feet above the river-level.

Of the plateaus between White and Yampah Rivers, Yampah plateau is the largest, and occupies an area of 400 square miles. The surface of the summit is undulating, and on the south side it presents a steep face, several hundred feet in height, covered with *débris*, rendering it almost inaccessible. This plateau is covered with excellent grass, and gives origin to numerous springs, all of which dry up within a short distance of their source.

As a whole, this district is very arid, barren, and almost destitute of tree vegetation.

The total number of stations made by Mr. Bechler in the

district assigned to him was 40, and the entire area was about 3000 square miles. Barometric observations were made whenever needed, and about 2000 angles of elevation and depression with fore- and back-sights, so that material for obtaining correct altitudes is abundant.

The rocks of this district embrace all the sedimentary formations yet recognised by the investigators who have studied the region that lies between the Park Range and the Great Salt lake, namely, from the Uinta quartzite (which underlies the carboniferous) to the Brown's Park group, or latest tertiary inclusive. Not only has the geographical distribution of these formations been mapped, but all the displacements of the strata have been traced and delineated. The last-named investigations bring out some interesting and important facts in relation to the orthographic geology of the region, especially as regards the eastern termination of the great Uinta uplift, and the blending of its vanishing primary and accessory displacements with those of the north and south range above mentioned. Much information was also obtained concerning the distribution of the local drift of that region, the extent and geological date of outflow of trap, &c.

The brackish water-beds at the base of the tertiary series, containing the characteristic fossils, were discovered in the valley of the Yampah. They are thus shown to be exactly equivalent with those, now so well known, in the valley of Bitter Creek, Wyoming Territory. These last-named localities were also visited at the close of the season's work, and from the strata of this horizon at Black Buttes Station three new species of *Unio* were obtained, making six clearly distinct species in all that have been obtained, associated together in one stratum at that locality. They are all of either distinctively American types or closely related to species now living in American fresh waters. They represent by their affinities the following living species:—*Unio clavus*, Lamarck: *U. securis*, Lea; *U. gibbosus*, Barnes; *U. metaneorus*, Rafinesque; and *U. complanatus*, Solander. They are associated in the same stratum with species of the genera *Corbulo*, *Corbicula*, *Neritina*, *Viriparus*, &c., and which stratum alternates with layers containing *Ostrea* and *Anomia*.

The close affinity of these fossil *Unios* with species now living in the Mississippi River and its tributaries seems plainly suggestive of the fact that they represent the ancestry of the living ones. An interesting series of facts has also been collected, showing that some of the so-called American types of *Unio* were introduced in what is now the

great Rocky Mountain region as early as the Jurassic period, and that their differentiation had become great and clearly defined as early as late cretaceous and early tertiary times. Other observations suggest the probable lines of geographical distribution, during the late geological periods of their evolutional descent, by one or more of which they have probably reached the Mississippi River system, and culminated in the numerous and diverse forms that now exist there.

The work of the past season shows very clearly the harmonious relations of the various groups of strata over vast areas; that although there may be a thickening or a thinning out of beds at different points, they can all be correlated from the Missouri River to the Sierra Nevada Basin. The fact also that there is no physical or palæontological break in these groups over large areas from the cretaceous to the middle tertiary is fully established. The transition from marine to brackish water forms of life commences at the close of the cretaceous epoch, and without any line of separation that can yet be detected, continues on upward until only purely fresh-water forms are to be found. Dr. White, an eminent palæontologist and geologist, says that the line must be drawn somewhere between the cretaceous and tertiary epochs, but that it will be strictly arbitrary, as there is no well-marked physical break to the summit of the Bridger group.

XUM

NOTICES OF BOOKS.

The Primæval World of Switzerland. With 560 Illustrations. By Prof. HEER, of the University of Zurich. Edited by JAMES HEYWOOD, M.A., F.R.S. London: Longmans and Co.

WE have here, under an unpretending title, a work so full of interest that the critic must pause, uncertain to what portions he ought principally to direct attention. The ordinary traveller who "does" Switzerland in the orthodox fashion, Murray in hand, or even the enthusiastic worshipper of Alpine scenery, has little conception of the wonders that lie beneath his feet. But to the enquiring mind of Prof. Heer the rocks have rendered up their secrets, and no portion of the world has been found more fruitful in evidence on the climatology and the geography, physical and organic, of bye-gone periods. The author before the appearance of this work was already honourably known from his "Fossil Fauna of the Arctic Regions," his "Swiss Tertiary Flora," and his "Plants of the Swiss Lake Dwellings." But in the volumes now before us he embodies the whole of his researches on the past development of his native country, and enables the student to examine the succession of plants and animals in the different geological formations, and thus to obtain information on weighty biological questions. As a matter of course the author belongs to the honourable class of monographists—observers who select some well-defined task and strive to master it fundamentally. But at the same time, whilst examining the Swiss fossilised annals of natural history, he does not neglect the light, complementary, explanatory, or corrective, which may be drawn from the changes of other parts of the globe.

The mere economical geologist, whose attention is exclusively confined to the occurrence of metallic ores and other marketable minerals, will not find in these volumes much to enchain his attention. Switzerland is not a mining country. True coal, with the exception of the anthracite beds of the Valais, can scarcely be said to exist. An argentiferous copper ore occurs in the Sernft Rock, and was worked on the Mürtschen Alp, in the canton of Glarus, from 1854 to 1861, but the mines have since been abandoned as unremunerative. The author remarks that during the Permian epoch conditions very favourable to the deposition of copper must have prevailed, whether by vapours laden with copper ascending from the interior of the earth, and depositing that metal in the rocks, or by salts of copper being

dissolved in water and precipitated and accumulated; but why it was precisely at the Permian period that the main deposits of copper originated is a question unsolved by the theory of aqueous deposition.

In the Brown Jura iron ore was formerly worked on the Oberlegli Alp, in the Klönthal, and in the valleys of Gentel and Lauterbrunnen, and at Gonzen, where it occurs along with black manganese ore, carbonate of manganese, heavy spar, fluor-spar, and iron pyrites. Petroleum is found in the lias marls to a great extent, but more in the Swabian Alp than within the political boundaries of Switzerland.

The carboniferous strata of Switzerland, limited as they are, bear witness to the very wide distribution of the plants of that epoch. The flora of the whole of Europe at that time was mirrored in the little island which formed the Switzerland of the carboniferous period. Even of the 300 species of coal-plants which have been found in America, about half occur also in Europe. This remarkable fact is not difficult of explanation if we consider that the flora of that epoch consisted chiefly of flowerless plants whose exceedingly minute spores would be easily swept away by the winds. Further, there was evidently a great uniformity of climate all over the globe, the chief amount of heat being due to the earth itself rather than that of the sun. The vegetable species in question would also encounter no opposition from social plants, such as grasses, which have since overspread so large a portion of the earth's surface; nor would they serve as food to animals—an exemption which their nearest allies enjoy to a very considerable extent even down to the present day. The carboniferous formation of Switzerland furnishes also the most ancient Swiss fossil animal with which we are acquainted—a species of cockroach (*Blatta helvetica*) nearly twice the size of the house-pest which has become naturalised throughout modern Europe.

The saliferous formation of Switzerland contains deposits of rock salt from 30 to 60 feet in thickness, and evidently due to a dried-up sea, as proved by the numerous bivalve shells found—the muschelkalk or shelly limestone. During this period Switzerland was covered by a shallow sea inhabited by encrinites quite foreign to the present European fauna; molluscs, approaching much more closely to those of our modern seas, and some of them even belonging to living genera. In Aargau Mösch has enumerated 57 of these species. Nautili, which now exist only in the Indian Seas, made their appearance in the carboniferous rocks long before the true ammonites. Fishes and reptiles inhabited the sea of the Triassic or Saliferous period. Remains of the *Ichthyosaurus* and *Nothosaurus* have been discovered by Mösch at Schwaderloch. The Keuper, or uppermost member of the Trias, is largely developed in the canton of Basel, where our author has obtained 25 species of fossil land-plants.

Thus we have proof that at the time of the Keuper there was in this region dry land, which probably extended over the Black Forest and the Vosges. The character of the flora is greatly changed from that of the Carboniferous period. The *Lepidodendra*, *Asterphyllitæ*, and *Sigillariæ* have disappeared, and in their stead we find gigantic horse-tails and Cycads of the genera *Zamia* and *Dion*, whose nearest living representatives are to be found in South Africa. In similar strata in Wirtemberg, Bavaria, and even in North Carolina, we meet with the same genera, and even the same species, as in the Swiss Keuper. Animal remains are also not wanting. Though Prof. Heer sought vainly for insects in the Keuper marls of Rütihard, he has found in the black shales of Vadutz two species of beetles (*Buprestites pterophylli* and *Curculionites prodromus*). We must here remark that the Curculionidæ and Buprestidæ are of all insects the most compact in their textures, as is well known to practical entomologists. The elytra of some of the species often turn the point of a pin, and require to be perforated with the point of a penknife or with a fine steel drill. Hence the remains of these beetles are much better calculated to resist either mechanical injuries or chemical changes than other insects, and we must therefore not be misled by their relative abundance in certain geological formations.

Huge reptiles inhabited Switzerland at this period: a gigantic *Labyrinthodon*, a salamandroid Batrachian, a crocodile (*Sclerosaurus armatus*), a large *Mastodonsaurus*, a *Belodon* "very like the gavial of tropical America,"* and certain *Teratosauri* have been found at Schambelen, in the Liesthal, &c.

The exploration of the Lias formation at Schambelen has proved exceedingly fruitful; nineteen fossiliferous beds, some containing marine and others terrestrial fossils, have been examined by Prof. Heer. He infers that the rock must have been formed in a quiet bay, protected from the agitation of the waves by a chain of hills projecting into the sea, or by a reef of rocks. It vividly reminded him of a scene he had witnessed in the quiet bay of Gorgulto, at Madeira. At Schambelen 22 species of plants and 182 of animals have been discovered. The latter consist of 1 reptile, 11 fishes, 143 insects, 6 Crustacea, 17 Mollusca, and 4 Radiata. The reptile, indicated merely by a tooth, was probably an *Ichthyosaurus*. Schambelen is the only locality on the continent of Europe where so many primæval insects have been preserved. In England the Lower Lias has indeed yielded 56 species, but they are in a worse state of preservation and less varied than those of Schambelen. The species found are distributed among the different "orders"—conventionally so-called—as follows:—Orthoptera, 7 species;

* This is a singular use of the name "gavial," which is generally restricted to *Crocodilia* of India and Australia.

Neuroptera, 7; Coleoptera, 116; Hymenoptera, Hemiptera, 12. Lepidoptera and Diptera are totally wanting, and the only Hymenopterous relic is a single wing. The author justly remarks that the hard elytra of the beetles may account for their relative numerical superiority, "but as the delicate membranous wings of the *Termites* have been preserved, there can be little doubt that if Lepidoptera had lived in the lias island, and fallen into the water, some traces of them would have been preserved." The absence of Diptera was a truly enviable feature of those days. Among the beetles the most striking feature is the relative abundance of the Buprestidæ, which we have to some extent explained; the paucity of weevils, due doubtless to the comparative scarcity of the seeds and fruits on which they feed; the total absence of long-horns, evidently not from a corresponding reason, as, like the Buprestidæ, they feed on wood; of ladybirds and of Brachelytra (devil's coach-horses). The scarcity of the Lamellicornes (dung-beetles, &c.) points to the want of ruminant animals, upon whose dung they prey. From a consideration of the plants and animals we may infer that the climate was at least semi-tropical.

In the Jurassic period "an immense sea extended over a great part of Europe, and in Switzerland only a few islands and coral-reefs rose above its surface." The islands of this period exhibit a flora having many links of affinity with that of the Lias, though the species are very distinct and deciduous leafy trees do not yet appear. The woody vegetation consisted chiefly of Conifers and Cycads, of species similar to some now flourishing in the southern hemisphere. The total number of species was small, as in the coral groups of the Pacific. The fauna of the Swiss Jurassic islands, as far as yet known, presents merely a few reptiles. If we complete the picture by a reference to contemporary deposits at Solenhofen, and in England and France, we find this period characterised by the appearance of the most ancient bird of the pristine world (*Archæopteryx macrura*), once regarded as a connecting-link between reptiles and birds. A dozen species of the Pterodactyle have been found at Solenhofen, and preyed doubtless on the gigantic dragonflies and large grasshoppers. The existence of dragonflies and of water-scorpions proves the existence of fresh-water lakes and pools. Longicorn beetles and Buprestids show that the islands were well wooded. The first Lepidopterous species, *Bombyx antiqua*, makes its appearance. The Jurassic strata of England have yielded remains of eighteen small mammals, belonging to the marsupial class which has in modern times its almost exclusive locality in Australia. But earlier traces of mammals have been found in the Trias of Richmond (Virginia), and in the Upper Keuper of Wirtemberg and England. The distribution of land and water in Central Europe at this period was remarkable. A large irregularly-shaped island comprised Bohemia, a narrow strip of

Central Germany, Baden, part of Eastern France, and Belgium. Central France, Holland, North Germany, the east of England, the middle of Switzerland, and the valley of the Danube and part of Northern Italy were the bed of a sea, whilst to the west a large island extended from Liverpool to the mouth of the Loire. After the Jurassic epoch, Prof. Heer is of opinion that *a transformation of the whole of organic nature had taken place.*

In the cretaceous period the central European island was greatly enlarged, and connected with Western England and Southern France, though the places where London, Paris, Vienna, Berlin, and Amsterdam now stand were still under water. The Swiss cretaceous marine fauna has two-thirds of its species in common with the Southern French (Mediterranean) sea, and only one-third in common with the Franco-Britannic sea. No remains of the land flora of this period have been hitherto discovered in Switzerland. From fossils found in other parts of Europe it appears that true leafy trees, such as poplars, figs, laurels, and myrtles, had now made their appearance.

In the Eocene period Europe must have possessed a nearly tropical climate and every condition favourable for the development of a rich fauna and flora. Evergreen forests of figs and Sapindaceæ, myrtles, and palms were inhabited by *Palæotheria*, musk deer and monkeys. Ferns have become scarcer than in the Cretaceous age, and Cycads have disappeared. The general character of the flora is Indo-Australian, though the peculiarly Australian types are less predominant. Mammalia are met with for the first time in Switzerland. There have been discovered 24 species of Pachydermata, 12 Ruminants, 1 Rodent, 8 Carnivora, and 1 Primate. All the species are distinct from those now living, and of the 25 genera represented only 4 have been handed down to modern times. One of the serpents was a python similar to those in India, and about 10 feet in length.

We arrive next at the Miocene epoch, and find the configuration of the European continent more nearly approaching its present state. Britain is united with France; Holland, Belgium, and Westphalia are still covered by the North Sea. The Mediterranean covers Egypt, spreads over Mesopotamia, and is probably directly connected with the Caspian on the one hand, and with the Indian Ocean on the other. Several isthmuses connected the European and the African coasts, and thus the African elephant, the hippopotamus, and the spotted hyæna are found in Sicily. The British Islands formed part of a great continent stretching across the Atlantic, and probably united to America. We thus perceive that Prof. Heer does not admit that vast antiquity of the existing oceans which many geologists and zoo-geographers uphold.

As far as research has gone Switzerland "takes the lead of all countries in its magnificent specimens of the Miocene flora, of which no fewer than 920 species have been collected within

the territories of the Republic." The country appears to have been a tropical forest region somewhat resembling the valleys of the Orinoco and the Amazon. Of many plants whose remains have not actually been found, there is indirect evidence in the presence of certain insects. The following passage is of a nature so instructive to the student that we quote it in full:—"Although the species of insects of the Miocene are distinct from those now living, they are frequently so nearly allied to them as to permit comparisons and inferences. Thus a *Galeruca* found at Œningen enables us to assume that pond-weeds existed there, and in the same way the presence of a forget-me-not is indicated by a small and elegant *Monanthia*, that of brambles by a *Syromastes*, that of nettles by a *Heterogaster*, that of a viper's bugloss by a *Pachymerus*, that of a trefoil by a *Clythra*, that of thistles by a shield-beetle as well as by a *Glaphyrus*, and that of a figwort by a *Cionus*. Other insects give us intimation of flowery meadows: the *Syrphi*, *Anthomyæ*, and *Malachii* no doubt sunned themselves upon the flowers, and the bees and humble-bees of Œningen collected their nectar just like their relatives of the present day. The dung-beetles also announce the presence of grassy meadows."

Œningen, Prof. Heer calculates, must have possessed a flora at least twice as rich as that of any Swiss region of similar extent at the present time. Eleven species of palms flourished in Switzerland, whilst only a single species—the dwarf fan palm—maintains a footing in the extreme south of Europe. Bignonias twined about the trees of the dense forests; species of cinnamon, laurels, myrtles, and magnolias were conspicuous. "If we bring together, according to their native countries, the living homologues of the Swiss Miocene species, we find that 33 species live in America, 16 in Europe, 12 in Asia, 3 on the Atlantic Islands, and 2 in Australia. If we extend the comparison to both homologous and analogous species, we obtain the following numbers:—Of the species most nearly resembling the Swiss Miocene species, 83 live in the Northern United States and 103 in the Southern United States, 40 in tropical America, 6 in Chili, 58 in Central Europe, 79 in the Mediterranean zone, 23 in the temperate, 45 in the warm and 40 in the torrid zone of Asia, 25 in the Atlantic Islands, 26 in Africa, and 21 in Australia."

Hence the floral affinities of Miocene Switzerland must be pronounced to be, in a predominating degree, American.

Of insects we have 876 Miocene species belonging to Switzerland or its immediate environs. Of these, 543 species are Coleptera, 81 Hymenoptera, 3 Lepidoptera, 64 Diptera. Of the latter the majority are gnats and midges. Among the beetles the Curculionidæ and the Buprestidæ* predominate. But though

* We note that the term "gold-beetles" is used as an English name for the Buprestids. It and the corresponding German word "gold-käfer" have hitherto been applied to the Cetoniadæ.

we are hence warranted in concluding that they were more numerous in the Swiss Miocene fauna than in that of the present day, it is, from reasons already mentioned, unsafe to pronounce on the proportion which they may have borne to other and more fragile families. No true Cetoniæ are known, but the allied genera *Trichius* and *Valgus* have occurred. All the Melolonthidæ are rare, but one species, *Lepitrix Germanica*, represents a genus now confined to the Cape of Good Hope. Two of the water-beetles found surpass in size any species now living. It is remarkable that one of the water-bugs found at Œningen is on a similarly gigantic scale. The Lepidopterous order is scarce at Œningen, as in all other localities for fossil insects. It is probably the youngest order, and was very sparingly developed in bye-gone epochs.

In the Miocene of Switzerland and Œningen we find 6 salamanders, 8 frogs and toads, 6 serpents, 21 tortoises, and 3 crocodiles. One of the salamanders of this epoch is the species described by Scheuchzer as "*Homo Diluvii Testis*."

Six species of birds have been recognised, and Prof. Heer possesses a fine fossil feather from Œningen.

Mammals abound, especially Mastodons, *Dinotheria*, five species of rhinoceros, the *Anchitherium* and *Hipparion*, genera forming a transition to the horse of the present time, eleven species of swine, and *Hynælorus Sulzeri*—a connecting-link between the cats and the hyænas, but considerably larger than the Bengal tiger. *Hyænodon* also combines the characters of the hyænas and the cats with some approximation to the marsupials. *Galecynus palustris* appears to connect the dogs and the civets. Tailless long-armed apes have also been found belonging to the most highly developed group of the "Quadrumana," an order which we regret to see Prof. Heer still retains.

From a careful consideration of the flora of Miocene Switzerland we may conclude that its temperature must have been very similar to that now enjoyed by Louisiana, North Africa, and the Canaries, with a mean annual temperature of 68° or 69° F. Even at Spitzbergen the Miocene flora resembles that now existing in Northern Germany, and indicates a mean temperature of 46° F.

But we must now take leave of the Miocene epoch, lingering traditions of which, if man existed so early, may have given rise to the "golden age" of mythology. Even before its close, however, a deterioration of climate became manifest, and in the next, or Quaternary period, the temperature of Switzerland and of Europe in general sunk to its present level. In the interval between the close of the Miocene and the formation of the Lignite, Prof. Heer thinks that the great western continent, which may have been the Atlantis of Plato, was submerged. Thus the connection between Europe and America was cut off, and perhaps the upheaval of the Alps took place at the same

time. This brings us to the contemplation of that awful phenomenon known as the Glacial epoch, or rather epochs; for two distinct attacks of glaciation, each lasting probably for thousands of years, are traced by Prof. Morlot from the mode of dispersion of the boulders of the Rhone Valley. In the interval between the two Glacial periods occurred, in Switzerland, the formation of the Lignites. Even this period has left traces of an important fauna. The remains of two huge oxen, the urus and the auerochs, both mentioned by Seneca and Pliny, can be traced to this period. Bones of the Irish elk have been found at Isteinerklotz. The woolly rhinoceros and the mammoth have been found in not a few Swiss localities, the latter of which made its appearance at the end of the second Glacial epoch. As to the existence of man in the Interglacial epoch, Prof. Heer regards the evidence as doubtful, as far at least as Switzerland is concerned.

Thus far we have given a brief and necessarily imperfect outline of Prof. Heer's view of the Palæontology of Switzerland from the Carboniferous to the Glacial epoch. It has been, of course, impossible for us to point out more than a mere fraction of the interesting facts here brought together, for no small portion of which the world is indebted to the untiring zeal and acute observation of the author himself,

Now, however, we enter upon the lessons to be learned from the phenomena placed on record—a matter where inference, not to say conjecture, is brought into play. It may here be needful to call attention to a peculiarity of the edition before us. It does not appear to be a literal translation from the original German, but bears rather the air—in certain passages at least—of an abstract or paraphrase. Indeed we are told, in the Preface, that both the German and a French edition were placed in the hands of Mr. Dallas for translation, and that the editor, Mr. Heywood, in revising the MS. was principally guided by the French version. This is to be regretted, since the responsibility of the opinions contained in the latter part of the work becomes thus divided. In our opinion the German edition should have been literally translated, and any valuable peculiarity of the French should have been added in the form of annotations or appendices.

Among the speculative questions to which the latter portion of this valuable work is devoted, we may select especially three —the causes of the glacial epoch, the first appearance of man, and the origin of species.

In the earlier periods, from the Carboniferous to the Tertiary, the earth's climate must have approached most nearly to that of the present torrid zone, and Prof. Heer finds no satisfactory evidence either of increase or decrease of temperature during this immense period; nor, until the Tertiary epoch, is a distribution of heat in zones perceptible. Even then the decrease of tem-

perature towards the poles was much less marked than at present.

As regards the causes of those alternations of temperature from the Lower Miocene to the Glacial, our author refers—but with only partial approval—to the influence of different distributions of land and water upon the earth's surface. The Arctic Ocean was probably, during the Glacial epoch, in connection with the Baltic, and the Sahara was—according to Prof. Escher de la Linth—submerged by the sea.*

Prof. Heer then examines four of the hypotheses advanced for the explanation of the phenomena in question. These are—Change of climate from the diminution of the earth's natural heat; modification of the sun itself; change of position of the earth with regard to the sun; and irregularity of temperature in ethereal space.

The first of these hypotheses would agree well with a constantly decreasing temperature, but it is utterly inadequate to explain a double period of intense cold with a milder epoch intervening, and succeeded by a more genial climate. Dr. Blandet's hypothesis, of a progressive decrease in the solar heat, is equally inadmissible. The third hypothesis, Mr. Croll's,† which seeks the cause of such alternating extremes of temperature in the periodically varying eccentricity of the earth's orbit, is rejected by our author on the ground that the plants and animals preserved in the rocks by no means confirm Mr. Croll's theory. The fact that the northern half of the globe has warmer summers than the southern atmosphere is ascribed to the different distribution of land and sea, the writer forgetting that the summer of the southern hemisphere is shorter than that of the northern, from the greater orbital velocity of the earth when in perihelion. Prof. Heer seems to lean to the fourth and last hypothesis, which seeks the cause of warm and cold periods in the temperature of different regions of space. "The Miocene period may be compared to the summer, the Glacial period to the winter, and the existing geological age to the spring of the planetary system."

But it is inconceivable that the temperature of different regions of space can vary, save as far as is due to the greater or less number and propinquity of stars radiating heat. Now if our earth, or rather our planetary system, passed so near such stars as to raise the average temperature of Europe by 16° F., their attraction would undoubtedly have exerted a very decided influence on the orbits of the earth, the other planets, and their satellites. Yet of such perturbations astronomers can find no traces. It will at once appear that, on the first, second, and fourth of these hypotheses, glaciation would occur in both the

* There is grave reason to fear that the schemes for letting in the ocean upon the Sahara might have disastrous effects upon the climate of Europe.

† Quarterly Journal of Science, vol. iv., p. 421, and vol. v., p. 309.

northern and southern hemispheres simultaneously, as Mr. Belt supposes. On Mr. Croll's view the two hemispheres would be subject to alternate glaciation.

The first appearance of man in Switzerland is traced, in an Appendix from the pen of Prof. L. Rütimeyer, of Bâle, to the Interglacial period. Pointed rods, carbonised along with the other constituents of the lignite, yet bearing evident traces of human labour, have been discovered among the coal from the Schöneich pit, at Wetzika, near Bâle. But if the first traces of man in Switzerland are thus shown to be Interglacial, his first appearance in the world is much more likely to have fallen in the Miocene epoch. Unless our race had been already widely distributed, and had made some advance from its rudest condition, it would probably have perished in the first Glacial epoch, under circumstances so unfavourable to any being of the order Primates.

Let us next turn to the question of the origin of vegetable and animal species.

Prof. Heer appears as an evolutionist, but as a non-Darwinian. He admits that during the Geological epochs, which he has so ably and graphically described, species have arisen, flourished, and passed away, giving place to others. These changes he does not ascribe to a series of miracles. On the contrary, he holds that new species are more likely to have been of organic than of inorganic origin, and even suggests that certain extinct species may have been the ancestors of modern forms inhabiting the same or adjacent regions. But he does not receive "natural selection" as the cause of the mutations of species. He denies that all living beings are necessarily and constantly undergoing a process of development, or a slow and uniformly progressive transformation. He considers that animals display a stability not only in their physical constitution, but also in their instincts, which he regards as decisive with reference to the continuance of specific characteristics.

It has been objected to this view that our observation of the habits and "instincts" of animals, especially of insects, is far too recent and too imperfect to enable us to decide whether these instincts are fixed or stationary. Prof. Heer seeks to elude the force of this argument by saying that certain species—*e.g.*, of ants in Switzerland—have precisely the same "instincts" as the same species in England or in Sweden. But as England has been separated from the Continent for probably 100,000 years it may be inferred that the instincts of these Swiss and English ants, both belonging to one species and derived from one common stock, must have remained stationary for at least such a length of time. To this certainly ingenious argument we may reply that the habits, and consequently the instincts of insects, at least in numbers of cases, are not specific, but common to entire genera, and even extend over larger groups. We need

therefore feel little surprise if, under circumstances mainly similar, we can perceive no modification, even during such long periods of time. Further, the sea does not absolutely prevent the passage of insects from the Continent to England. Hornets, wasps, winged ants, &c., may cross a strait of 20 miles in breadth as readily as butterflies and locusts, both of which have been known to fly much greater distances. We may further point to distinct instances where "instincts" have been found capable of variation, and where changes have been made in accordance with modifying circumstances. In a book which is in the hands of every naturalist* we find instances of alterations and improvements in the nests of birds.

Prof. Heer maintains that "if instinct were the result of education it would at the same time be capable of attaining perfection, and, in the case of insects gifted with the most wonderful instincts, changes might be expected more rapid in consequence of the very limited period of individual existence of each insect." Now, this "very limited period of individual existence" is, we maintain, precisely the reason why changes are so slow. The longer any animal lives the greater opportunity it has to profit by experience, and the more probability there is that its fellows can observe and profit by any improvement upon which it may have comé. If we consider how slow has been the progress of man—how often even the most highly-organised varieties of our species have been at a stand-still, or even retrogressive—need we wonder that in animals far lower the movement may escape our observation?

Prof. Heer considers that we are still in the dark as to the fundamental conditions of the transformation of types which he admits to have taken place. Believing the changes to be sudden instead of gradual, he approximates to the position of Prof. Mivart.

The following passage from the Editor's Preface should not be overlooked:—

"In Ancient Tertiary strata the chestnut (*Castanea atava*) had leaves distinctly toothed, but devoid of any points, and from the primary vein of the leaf curved secondary veins sprang at a distance from each other.

"In the Middle Tertiary strata the secondary veins of the chestnut leaf approximated to one another, and teeth protruded and were more numerous.

"In the latest Tertiary strata the secondary veins of the chestnut leaf were still nearer to each other, and almost rectilinear, while the teeth of the leaf had become set with thorns, as in the sweet chestnut (*Castanea vesca*) of our own times."

Is not this a striking case of that gradual transformation which Prof. Heer is unwilling to recognise?

* WALLACE, on Natural Selection, pp. 224 to 230.

The edition before us is abundantly illustrated. There is a geological map of Switzerland; there are a series of small charts showing the arrangement of land and water in Central Europe during the successive epochs; there are a number of representatives of the fossil vegetable and animal forms described in the text; and, lastly, a set of imaginary landscapes, showing the characteristic fauna and flora of Switzerland at each period.

Tonquin. Report by Sir B. ROBERTSON (Her Majesty's Consul at Canton) respecting his Visit to Haiphong and Hanoi, in Tonquin. 1876.

So little information has been made public in recent years respecting the almost unknown kingdom of Tonquin, in the Indo-Chinese peninsula, which, through the intervention of the French, has just been partially opened to foreign intercourse, that we venture to think a brief *resumé* of some portions of this very interesting Report to the Foreign Office may not be unacceptable to our readers.

We must premise that last spring Her Majesty's Minister at Peking directed Sir Brooke Robertson, H.M.'s Consul at Canton, an officer of long standing and great experience, to visit Haiphong and Hanoi, the latter the capital of Tonquin, to ascertain their commercial capabilities. Haiphong is situated on the right bank of the Cua-cam, a branch of the Song-koi, or Red River, on which is Hanoi at a distance of 145 miles from the sea. Haiphong is connected with Hanoi either by the River Cua-cam or by canals: after proceeding some 25 miles by this route boats turn into another canal, and after going some 77 miles enter the main river, the Song-koi, and so on to Hanoi, a distance of about 45 miles. At Haiphong there was little to be seen and not much to be learned, for what business there is is done at the capital, where the native and Chinese merchants all reside, and thither Sir B. Robertson proceeded as soon as he could make arrangements for the journey in a small steam-launch. For some time but little was to be seen of the country, the banks of the canal being too high; but from what could be seen it appeared to be fertile, and planted with maize and sugar-cane, sweet potatoes, and other products of those latitudes. On debouching, however, into the Song-koi, a better view was obtained, and there the bamboo and other trees, with villages and fields, made together a scene of much natural beauty, and gave evidence of a high state of cultivation. There is a fair amount of boat and junk traffic; from the stake-nets, &c., fish evidently abounds; rafts of wood and bamboo were frequently met with; and altogether there was evidence of a contented and well-to-do

population. From December to May the Song-koi, or Red River, is very low, but about May the melted snow and ice from the mountains of Upper Tonquin and the Chinese province of Yünnan come down, and the river rises rapidly, attaining a height of 30 feet, and often carrying away the banks and flooding the country.

The approach to the city of Hanoi, the capital of Tonquin, is very fine, for a turn of the river—at that point over a mile in breadth—brings into view the town and the settlement, on the construction of which the French are expending much care. The city rises gradually from the river, and, being embedded in trees and foliage, it has a charming appearance on a bright and sunny day. That part of Hanoi where the citadel—so gallantly captured by Lieut. Francis Garnier on November 30th, 1873—is built is somewhat higher than the other quarters of the city, the ground rising gently from the banks of the Song-koi to the height of about 180 feet above the level of the sea. A brick wall, about 3 feet thick and 12 feet high, comprising twelve bastions connected by curtains, surrounds the citadel, which forms a perfect quadrilateral of some 3600 feet on each side. Inside this citadel are the houses of the Governor and other high officials. Besides these and some barracks, there are but few houses; in the centre there is a very peculiar tower on a raised base, some 20 feet high, and on the top of this tower the national flag is hoisted.

The town of Hanoi—the population of which, we may note in passing, is estimated at from 150,000 to 200,000 Tonquinese and about 2000 Chinese—is situated between the citadel and the river, and extends beyond the former in a westerly direction. The streets are wide, and the houses good and well-built of brick in some of the principal thoroughfares, but there are numerous streets where the shops are merely mat erections, in the usual Tonquinese style. With one exception—the main street from the river, where the Chinese live—the streets are unpaved, and in wet weather almost impassable from the depth of the mud; but as shoes are an exception with the natives, this drawback to European comfort does not affect them.

The sedan-chair of China—so convenient for locomotion—is unknown, the high authorities and those who can afford it being carried in a hammock of silk or hemp net-work, suspended on a pole from the shoulders of two or four bearers, and closed with silk or cotton curtains. There is a great deal of stir and movement in the streets; the shops are well-stocked with articles of native workmanship—especially bamboo-work in all its varieties; silk and cotton piece goods; paper, pewter, and glass ware; boxes; drums of all sizes, richly painted, lacquered, and gilt; and coarse crockery and china-ware. Fowls, ducks, geese, oxen, and pigs are both plentiful and cheap, with abundance of fruit, bananas, vegetables, and fish. Rice is the staple food of the

people, who look well-fed and healthy, and have a fair amount of muscular development. The women, however, appear to be the dominant class, for they work like the men as coolies and labourers, and preside in the shops. They are generally good-looking, carry themselves gracefully, and, but for the practice of blackening their teeth, many might be called handsome. In manner the Tonquinese are quiet, and, owing perhaps to the arbitrary government they are under, subdued and indolent. Mr. N. B. Dennys, who visited Tonquin last spring as the delegate of Hong-kong General Chamber of Commerce, gives us, in his Report, some more details respecting the women of Tonquin, and it will be seen that he is at variance with Sir Brooke Robertson on the subject of their good looks. "The dress of the women," he writes, "is picturesque, their figures are good, but their faces, as a rule, are by no means pretty, and very dirty-looking. They wear trowsers, with a square piece of silk over the breast, fastened behind. The better sort wear a long dress over this. Their heads are covered with cloth turbans, with the hair so arranged as to seem as if it was in a small net at the back. The teeth of both men and women are, as amongst other betel-chewing races, perfectly black, and their lips a bright red. The ordinary dress of the men is a long coat and trowsers, with a conical hat, from either side of which hang long pieces of silk."

Returning to Sir B. Robertson's Report, we note some of the leading geographical features of the country. The principal river in Tonquin, the Ho-ti-kiang,—better known now as the Hung-kiang in Chinese and the Sung-koi in the native language, both names meaning Red River,—has its rise in Thibet, and is navigable from Mang-hao, the last city in the Chinese province of Yünnan, down to the sea, a distance of 414 miles. Mang-hao is the *entrepot* where goods are shipped to and from Tonquin, the mart being a more northern city, called Mang-tsze, which is also situated on the banks of the same river. The Sonk-koi divides into various branches, the two southern ones meeting above Hanoi; on one of them stands the town of Niuh-brinh, and on the other Nam-diuh and Hung-yen. These two branches communicate by canals, on one of which is situated the town of Fouli. It is not necessary further to describe the various branches, &c., of the Song-koi; but as it may become of very great importance, if the French ever succeed in opening up a trade with South-Western China, which Sir B. Robertson evidently considers by no means an impossibility, the following remarks of his are not without interest:—"I saw the peculiarly constructed boats, of great length and drawing little water, employed in ascending the river as far as Mang-hao and Mang-tsze, both large trading cities in Yünnan, and I closely questioned one or two people who had been that route, and they stated that no difficulties existed. That the River Song-koi is available,

therefore, for the transit of the reported rich ores and produce of Yünnan and Tonquin, seems certain, and on the removal of three obstacles alone a large and profitable trade may be said to depend, viz., the consent of the Governments of Tonquin and China to open the mines, and the clearance of the frontier of refugee Chinese and leviers of black-mail. All these accomplished, the rest is easy. If the Song-koi, or Red River, opens a route to Yünnan and Kwangsi, that *via* the Shan States and Bhamo is not the less available, and the two offer equal advantages, nor is there any reason why their interests should clash; on the contrary, for these points of exit are distant, and one gives geographical facilities which the other does not."

Sir B. Robertson found it difficult to obtain information regarding the commercial products of Tonquin; but he met with a native, called Patrus Trueong Vinkky, who was an *employé* of the French Government at Saigon, and had been sent to enquire into the trading resources of the country. He represented Tonquin to be rich in products of all kinds, having coal, copper, and tin mines, as well as gold and silver. Its agricultural capabilities are immense, and silk is abundant and cheap,—in fact, not much more than one-third of the price it is in China: it appears, however, at present to be much coarser in thread than the Chinese, and very badly reeled, and the fabrics made from it are of very poor quality. From all he could learn, Sir B. Robertson concluded that Tonquin was a very rich country, and, with the establishment of good relations with the Government, might be made a very profitable one to western nations.

The Theory of Sound in its Relation to Music. By Professor Pietro Blaserna, of the Royal University of Rome. London: Henry S. King and Co.

This volume of the International Scientific Series will be acceptable to all musicians who wish to know something of the manner in which sounds are produced, and the laws regulating them. The author has combined his information in the most happy manner, and produced a work useful both to the students of physical and musical science.

The earlier chapters treat on vibrations, and the various well-known experiments for their demonstration. After discussing the production of sound by means of the vibration of solid bodies, the subject of sounding-pipes is explained, and their application in the construction of wind instruments. The description of the manner in which a single elongated tube is able to produce several notes, by overblowing, aided by opening communications with the external air by means of keys, as in the flute and clarionet, is correct; but the singular error has arisen

of attributing the same action to the pistons of brass instruments which do not open holes in the tube, but by means of valves add additional lengths of pipe, and so produce different fundamental notes, the harmonics of which are formed by the lips and pressure of breath of the performer.

Fuller information respecting the structure of musical instruments would have been very acceptable to a large class of readers, especially as information on this subject is not easily attainable.

Several chapters are devoted to subjects almost entirely musical, such as ratios, consonant and dissonant chords. The nature of scales, ancient and modern, is explained in considerable detail. The "Equal Temperament" does not find much favour with Prof. Blaserna: he accepts it as a necessary evil, but considers that a key-board of twenty-four notes to the octave would just be able to produce a satisfactory effect.

The chapter on *Timbre*, or quality of sounds, is particularly interesting, and contains descriptions of the *Phonautograph* of Scott, and the equally valuable *Harmonic Detector* of König.

Histological Demonstrations, being the Substance of Lectures delivered by GEORGE HARLEY, M.D., F.R.S. Edited by GEORGE T. BROWN, M.R.C.V.S. Second Edition. London: Longmans and Co.

WORKS containing the practice of accomplished manipulators in any branch of Science are always welcome, and the present little book is no exception. The carefully edited notes of Mr. Brown have in this edition had the advantage of Professor Harley's revision, and the result is a collection of practical information of the utmost value to medical and veterinary students.

The portion of the work devoted to the description of the instrument, its use, and the general processes employed in the preparation of tissues for microscopical examination, is somewhat small, only twenty-seven pages being devoted to these subjects; however, this department has been so fully treated in the copious works of Beale, Carpenter, and others, that greater extension was hardly needed, and more space has been left for the special subject of the work.

Of the treatment of this part it is hardly possible to speak too highly.

The tissues, from the most elementary to those of the highest elaboration, are described briefly, yet with sufficiency of detail to aid the student; special directions are given as to the best mode of manipulating, and a diagram or figure given with each, to call attention to what should be observed; no less than two

hundred and twenty woodcuts are inserted in a book of two hundred and seventy-four pages.

The examination of diseased tissues is fully demonstrated. This portion of the work will be useful not only to the student, but may be consulted with advantage by the medical practitioner, who will find, in a compact form, information which would otherwise require a search in many and not easily procured publications.

Animal and vegetable parasites have also a fair space devoted to them.

The woodcuts are well executed; the only defect of the work is the absence of references to authorities where further information is to be obtained.

A Letter addressed without permission to the Astronomer Royal, explaining a New Theory of the Solar System, and placing Newton's Theories upon a Physical Basis. By G. T. CARRUTHERS, M.A. London: Longmans and Co.

WE cannot give the reader a better idea of the views advanced in this little work than by quoting from an abstract which the author has appended:—

"The earth and planets are supported by the atmosphere or its watery vapour, in the same way as the weight on a safety-valve is supported by the steam in a boiler. The rotation of the earth is caused by the impacts of the atmosphere upon the earth, *i.e.*, by a constant series of blows from the sun of 2116 lbs. upon every square foot; the number of feet passing in a second being, as we know, enormous.

"The orbital motion of the earth is caused by the rush of cold air at night towards the sun, as the air in a room rushes towards the fireplace. Magnus has shown, by experiment, that such a rush of air upon a revolving cylinder causes it to move in a lateral direction, similar to that in which the earth moves.

"The issue of watery vapour of very rare density takes place from the North Pole, where there is little rotation. The atoms of vapour gradually accumulate and form condensations of vapour at great distances from the North Pole, when the force with which they were expelled is expended. Thus the earth has formed the moon, and the sun the planets.

"The atoms when condensed are urged downwards to the Equator, and receive there the rotary impacts of the sun's vapour, which compel them to rotate in the same direction as the sun. The planets may thus be supposed to be at the centre of gravity of great cones extending northward, which rest upon the great cone of the sun's vapour. Hence the motion of the

earth may be likened to that of a ball suspended from a string and twisted around, and the earth goes upon its orbit as one cone rolls round the surface of another. This theory explains, satisfactorily, therefore, the observed motion of the earth, which seems to be going forward by twisting itself backwards in a way quite different to our common notion of the manner in which a sphere or carriage-wheel should roll onwards.

"The cones are masses of vapour at its greatest density for the temperature, of sufficient strength and power to sustain the planets in their places, and the distances of the planets accord with the volumes of steam which sustain similar weights.'

The Puzzle of Life and how it has been put together, a Short History of Vegetable and Animal Life upon the Earth from the Earliest Times. By ARTHUR NICOLS, F.R.G.S. London: Longmans and Co.

WE have here an elementary treatise on geology and palæontology adapted to the comprehension of young children. The language is plain, the descriptions are lucid, the illustrations apt, and the broad facts of the science are very correctly stated. The work, too, is perfectly free from all attempts at fine writing, and from those peculiar passages we occasionally meet with which prove that their author is thinking more of himself than of his subject. Trains of argument are of course absent, and the thread of the subject is not overlaid with a perplexing multitude of details. Nor, on the other hand, has Mr. Nicols deemed it judicious to obscure his theme with "the mist of romance." He remarks, and we believe quite truly, that "children soon begin to look with contempt on fiction when applied to natural philosophy, and I would not consciously impose on them the grave task of unlearning anything." But whilst we feel bound to express our approbation of the manner in which the author has carried out his undertaking, we cannot help expressing a doubt as to the reception the book will meet with among children of an average class. From the Dedication and from some expressions in the Preface we should infer that the author has brought the subject-matter of these pages before a number of children in *viva voce* lessons or conversations, and has succeeded in awakening their interest and in reaching their understanding. But children are confessedly reached much more readily in this manner than through the medium of printed paper. We wish the book success as at any rate an attempt to lay before the young fact instead of fiction,

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Problems and Examples in Physics. An Appendix to the Seventh and other Editions of Ganot's "Elementary Treatise on Physics." London: Longmans and Co.

THE author of this pamphlet, Mr. E. Atkinson, remarks in his Preface that—"The present book, consisting of a series of numerical problems and examples in Physics, is based upon a similar one contained in the French edition of the work. But I have been able to use only a small proportion of the problems contained in that Appendix, as the interest of the solution was in most cases geometrical or algebraical. Hence I have substituted or added others, which have been so selected as to involve in the solution a knowledge of some definite physical principle."

We consider the substitution thus referred to by the author a decided improvement.

The Applications of Physical Forces. By AMEDEE GUILLEMIN. Edited by J. N. LOCKYER. London: Macmillan and Co. 1877.

THIS forms the companion volume to the "Forces of Nature" of M. Guillemin, a translation of which appeared in this country five years ago. The present work is published in the same form, with numerous and elaborate woodcuts and coloured plates: the one deals with pure science; the other with some of the more prominent applications of the sciences.

The work is divided into five books, which treat respectively of the applications of the phenomena and laws—1. Of Weight. 2. Of Acoustics. 3. Of Light. 4. Of Heat. 5. Of Magnetism and Electricity.

We must take exception to the title of the first book: to speak of the phenomena of *weight* when we mean the phenomena due to a force, one of the effects of which is to produce the effect we call "weight," is surely an example of misplaced diction.

This book treats of plumb-lines and levels, pile-drivers, the pendulums of clocks, and balances. In the second section, of the hydraulic press, hydrometers, liquid levels, pumps, fire-engines, and air-pumps. The atmospheric railway is described, but no mention is made of the use of air-pumps for exhausting vacuum-pans. The applications of compressed air comprise the air-gun (which is surely the least important application) and the boring of tunnels by a compressed air-engine. Compressed-air railways are beginning to make their appearance in various cities: in Paris small parcels are conveyed from the Grand Hotel to the Place de la Bourse at the rate of 40 feet a second. The pressure of air is produced by the pressure of water (equal

to 15 metres of vertical height) in the reservoirs of the city. In London there are twenty-five pneumatic tubes representing a length of 18 miles. The tubes are 2¼ inches diameter, and are worked by alternately compressing and exhausting air by means of three steam-engines, each of fifty horse-power. In New York passengers are conveyed by the same means.

A curious misprint, twice repeated, occurs on p. 81; Hero's Fountain is printed *Nero's* Fountain: the same error occurs twice on the following page. A chapter on ballooning concludes the first book.

In the second book, on Acoustics, we have a useful section on "Acoustics applied to Architecture;" then a description of various musical instruments, ancient and modern, including a most capital account of the construction of the violin, "the most perfect of musical instruments. The back and ribs of the violin are made of a hard close-grained wood, usually beech; the upper plate or belly is made of a light wood, such as deal or cedar. The old violin-makers preferred Swiss pine for the belly and maple for the back. An effective woodcut (fig. 96) shows the various parts of the instrument. Although, as is well known, violins improve considerably by age, our author very justly remarks—"But it must not be forgotten that the beauty of the tone of an instrument of this kind depends, in great measure, on the talent of the artist in whose hands it may be. Nearly his whole skill, from this point of view, lies in regulating the pressure by which his right arm, or more properly speaking his right hand, directs the bow, and the clearness and force with which the fingers of the left hand press the string." The complex mechanism of the harp is well shown in fig. 109. An interesting account of the great organ at Primrose Hill, built by Bryceson Bros., is given in Chapter 5. This fine instrument embraces seven distinct organs, and the bellows and "vacuum pressure" are worked by an eleven horse-power steam-engine. There are seventeen compressed-air reservoirs, and two vacuum reservoirs. The echo organ—100 feet distant from the keyboard—is worked both by electricity and by vacuum pressure, that is, the pressure produced by air striving to enter a vacuum. The 32-feet pipes in the great organ in Notre Dame each require 70 litres of air a *second* to sound them, while the whole mass of compressed air in the organ amounts to 25,000 litres.

The third book treats of the applications of the laws of Light, and first of mirrors and reflectors, and of instruments in which mirrors are used, such as the sextant and goniometer, the heliostat and siderostat. A good account of lighthouse appliances follows: figures are given of the azimuthal condensing prisms used in the Sound of Skye, and of the peculiar form of lens used in the Tochindall lighthouse.

The chapter on the Microscope contains some beautiful coloured drawings, among which we may specially call attention to the

distribution of blood through the brain, and the section of the retina of a bird. A brief account of photo-lithography and chromo-heliography conclude this book.

The fourth book, on the applications of the laws of Heat, commences with a discourse on the art of warming, naturally followed by a comparison of different kinds of fuel. The steam-engine, in all its phases, receives very full treatment, and these seem to us to be the best chapters in the book—the most complete and comprehensive, and most carefully written and edited. A description of the condensed-air locomotive which is used in the St. Gothard tunnel, and of several kinds of road locomotives, concludes this part of the subject.

In regard to the application of steam to printing, it is mentioned that the first sheets printed by steam were struck off in 1814. Ten thousand copies per hour were struck off. Till lately the "Hoe ten-feeder" was the most rapid printing-press in existence: it can turn out 8000 copies of perfect newspapers per hour, but requires eighteen people to attend to it. The Walter press, however, by means of which "The Times" is printed, requires the attention of one man and two boys, and prints 12,500 copies per hour.

Some interesting statistics of steam-engines and railways will be found on p. 498. From these we learn, on the authority of Fairbairn, that the steam-power employed in England alone is equal to 3,650,000 "horse-power,"—equivalent to the labour of 76,000,000 men. In France there were in 1865 steam-engines having a united power of 242,209 horse-power, not including 4000 locomotives. At the end of 1876 the railways of the world had reached a total length of 176,141 miles, which is nearly seven times the circumference of the earth. The following numbers show the distribution of the mileage:—

Europe	83,864
America	82,335
Asia	6,822
Africa	1,675
Australasia	1,463

The next statement is somewhat surprising:—Out of 100,000 ships in Europe forming the mercantile marine, 4500 ships employ steam—a much smaller number than we should have imagined. The 100,000 must surely include a large number of coasting-vessels and other small craft. At the same time sailing-vessels are being very rapidly converted into steam-vessels; and while in 1861, out of 975 new ships, only 207 were fitted with steam, in 1874—out of 981 new ships—no less than 482 were steam-ships. In 1874 we had no less than 109 steam-vessels in the Navy, and of these sixteen were ironclads.

The fifth and final book treats of the applications of Magnetism and Electricity, and this constitutes more than one-quarter of

the entire work. Commencing with an account of the compass and dipping-needle, the subject of lightning-conductors is next discussed, and this is followed by the great subject of electric telegraphy. This receives very ample discussion, and all the newest improvements are described. The electric light is now employed far more generally than it used to be, and apparently it is far from expensive. The electric light which was employed in the clock tower of the Houses of Parliament was produced by a Gramme machine driven by a two horse-power engine at a rate of 320 revolutions per minute. It is stated that it produced "a light equal to 7000 sperm candles, at a cost of about one shilling per hour." The electric light, which is employed for many stage effects, at the Grand Opera in Paris is produced by 360 Bunsen elements. The application of this intense light to the lighting of towns has not proved a success: it is probable, however, that if a great elevation could be secured, it might be used with advantage. It would be interesting to try the effect of the light on the top of Strasburg Cathedral. A few general applications on electricity conclude the fifth book and the volume.

This new work of M. Guillemin is a useful adjunct to his former volume on the physical forces; it does not, however, present the same features of novelty to the general reader, because the former volume, on account of its popular nature, could not well avoid allusions to some of the practical applications of the forces which it described.

Acoustics, Light, and Heat. By WILLIAM LEES, M.A. London and Glasgow: William Collins and Co. 1877.

MESSRS. COLLINS are doing good service to the cause of scientific education by issuing a cheap and generally excellent series of text-books. There are two series of such books—an "Elementary" or Primer Series, and an "Advanced," usually by the same author. The elementary volume of the present work has been published for some length of time, and has been found a fairly useful book for science classes in schools. The work before us embraces all that can be required by the advanced classes in schools; it is well illustrated, and in the main clearly written.

An Introduction to the Theory of Electricity. By LINNÆUS CUMMING, M.A., Assistant Master at Rugby School. London: Macmillan and Co., 1876.

THIS work embraces in a compact and well-arranged form many of the mathematical results which have been obtained by Prof. Clerk Maxwell and others. It commences with a chapter on "Physical Units," in which we find definitions of *Velocity*, *Acceleration*, *Density*, *Force*, *Momentum*, *Moment of a Force*, *Couple*. The latter is defined as follows: "Two forces which are equal in magnitude and parallel, acting in opposite directions, but not in the same straight line, are termed a *couple*." Work is defined as "resistance overcome through space." Kinetic energy as "half the product of the mass into the square of the velocity of a body." The second chapter is on the theory of the *potential*. The potential at a point is defined as "the work done in carrying a gramme from that point to infinity." The expression "difference of potential" is frequently used in electricity, and its precise meaning is this: that if the potential of any two attracting systems be measured, the difference represents the work done on a unit of mass moved from one point to the other. *Tubes of force* are "tubular surfaces bounded by lines of force." The ideas developed in this chapter are next applied to certain experimental results; such as the proof that electricity resides entirely on the surface of bodies. A number of problems both in Statical and Voltaic Electricity follow; Ohm's Law is discussed at great length, and finally we have an application of some of the preceding laws to magnetism. The work will be welcomed by all students of electricity. It supplies a distinct want, and supplies it well.

A Treatise on the Kinetic Theory of Gases. By HENRY WILLIAM WATSON, M.A. Oxford: Clarendon Press. 1876.

THIS small work gives us, in a condensed form, the main results obtained during the last few years by Clausius, Clerk-Maxwell, and Boltzmann. It is from first to last mathematically treated, and will only be useful to the reader well acquainted with mathematical modes of thought. It is included within fifty-one pages, and embraces thirteen propositions with their proofs. We may take the tenth proposition as an example:—"A homogeneous gas being supposed to be constituted of moving molecules with any given number of degrees of freedom, required to find the ratio of the specific heat at constant pressure to that at constant volume."

Chemical and Physical Researches. By THOMAS GRAHAM, D.C.L., F.R.S. Collected and printed, for private circulation only, by JAMES YOUNG, F.R.S. and Dr. ANGUS SMITH, F.R.S. Edinburgh: 1877.

IT would have been difficult to offer more graceful evidence of friendship, and nothing would have touched and gratified Mr. Graham more, than the publication of this volume by his valued friends Mr. Young and Dr. Angus Smith.

Mr. Graham's work was peculiar, as his researches were mainly devoted to the elucidation of questions which occupy an intermediate position between chemistry and physics. Dr. Smith therefore wisely determined not to place the papers in strict chronological order, but to arrange them under three divisions, headed respectively "Gases," "Salts and Solutions," and "Unclassified Papers." The plan is a most fortunate one, for it well shows the gradual development of Mr. Graham's thoughts in each division of his work, and at the same time it exhibits its strength and coherence.

His earliest paper "On the Absorption of Gases in Liquids," published in 1826, is remarkable for ingenuity and close reasoning. In it he considers that "gases may owe their absorption in liquids to their capability of being liquefied, and that when gases appear to be absorbed by liquids they are simply reduced to that liquid inelastic form which otherwise, by cold or pressure, they might be made to assume; their detention in the absorbing liquid is owing to that mutual affinity between liquids which is so common. Faraday had shown, in 1823, that in the physical states of gas, liquid and solid, there was nothing of absolute permanency, and that any body may assume consecutively all these forms." Hence Mr. Graham concluded that those bodies which at the temperature of the atmosphere we experience to be gases, may be considered, without impropriety, as volatilised liquids, and he pointed out that it was not necessary that gaseous bodies —whose absorption in liquids he was explaining—should be presented to the liquids in a liquefied state, for the mere absorption of such gases by liquids occasioned their liquefaction. He gives as an instance the liquefaction which must accompany the absorption of steam, at 600° F., by sulphuric acid heated to the same temperature, while in order to liquefy the gaseous body in the ordinary way it would be necessary to cool it down through a range of nearly 400°. The absorption is, in fact, dependent upon "the affinity which occasions the miscibility of two liquids." In his last paper in the "Philosophical Transactions," published more than forty years afterwards, he refers to the liquefaction of *gases in colloids* in much the same terms, for he alludes "to the general assumption of liquidity by gases when

absorbed by actual liquids or by soft colloids," and he shows that those gases penetrate rubber most readily which are easily liquefied by pressure, the rates of passage of carbonic acid and hydrogen being 11·81 and 4·73 respectively; that gases undergo liquefaction when absorbed by liquids and such colloid substances as india-rubber; and finally, that the complete suspension of the gaseous function during the transit through india-rubber cannot be kept too much in view.

Probably the subject with which Mr. Graham's name will always be specially connected is the diffusion of gases. The first paper on it was published in the "Quarterly Journal of Science" for 1829. He found that the lighter a gas is the more quickly it diffuses away from an open cylinder, and the experiments led him to believe that the diffusiveness of gases is inversely as some function of their density, apparently the square root of their density. In this long series of researches the molecular aggregation of the septa through which the gases penetrated was always carefully considered, and in dealing with gaseous penetration he clearly saw that orifices of excessive minuteness might be quite impassable by gases of low diffusive power; that in other cases pores of graphite could only be permeable by *molecules;* and lastly, as will be subsequently shown, that there might be an inter-molecular porosity due entirely to dilatation at a high temperature.

In a paper published in 1831 he established the following law of the diffusion of gases:—The diffusion or spontaneous intermixture of two gases in contact is effected by an interchange in position of indefinitely minute volumes of the gases, which volumes are not necessarily of equal magnitude, being in the case of each gas inversely proportional to the square root of the density of that gas. He speaks of diffusion being effected by a force of the highest intensity, and urges that sensible masses are not affected by diffusion, but only molecules. Diffusion afforded, in Graham's hands, the most conclusive proofs of molecular movement, and as an instance of the rapidity of such molecular mobility it should be remembered that Maxwell has estimated that the initial velocity of a molecule of hydrogen is nearly 1860 metres per second.

The continuation of the researches led him to study the passage of gases through a minute orifice in a thin disc of platinum, a mode of passage, termed *effusion*, which left no doubt of the truth of a general law, that different gases pass through minute orifices in times which are as the square roots of their respective densities, or with velocities which are inversely as the square roots of their respective densities. The research afforded an experimental verification of the truth of the mechanical law, that the velocity with which a gas rushes into a vacuum, through a minute aperture, is the same as that which a heavy body would acquire in falling from the height of an atmosphere composed of

the gas in question of uniform density throughout. Mr. Graham carefully points out that the phenomena of diffusion and effusion are essentially different, although the rates of passage are alike; *diffusion* only permits molecules to pass, but masses pass by effusion.

He found that if the orifice in the platinum disc became tubular, the passage of carbonic acid and nitrous acid became quicker in relation to air than when the rates of passage through a thin punctured disc were examined, and from facts such as this a new kind of passage was detected, which he called transpiration. By employing capillary tubes the effusion and transpiration rates of passage were found to differ widely, "for if the length of the tube is progressively increased, and the passage of all gases becomes greatly slower, the velocities of passage of the different gases are found to diverge greatly from their effusion rates." The velocities at last, however, attain a particular ratio with a given length of tube and resistance, and preserve the same relation to each other with greater lengths of tube and resistances; the most simple result being probably that of hydrogen, which has exactly double the transpiration rate of nitrogen, the relation of these gases as to density being as 1 : 14. Mr. Graham viewed transpirability as being a kind of elasticity, depending upon the absolute quantity of heat which different gases contain under the same volume, and it is therefore more immediately connected with specific heat than with any other property of gases. One very important branch of Mr. Graham's labours appear to have commenced in 1829, with a notice of the "Singular Inflation of a Bladder," which was two-thirds filled with coal-gas, and on being introduced into carbonic acid for some hours became distended. Mr. Graham pointed out that, although Dutrochet would probably view in the experiment the discovery of endosmose acting upon aëriform matter as he had observed it to act upon bodies in a liquid state, the penetration of the carbonic acid was really preceded by an actual absorption in the water with which the pores of the bladder were filled. He continued the research at intervals and published a series of papers a few years before his death, in which he showed that the penetration of gases through colloid septa, such as india-rubber, was the result of an actual occlusion or absorption. A comparison of the relative rates of the penetration of oxygen and nitrogen through india-rubber led to a most remarkable experiment. Oxygen penetrates $2\frac{1}{2}$ times as fast as nitrogen; therefore by dialysing air Mr. Graham actually increased the quantity of oxygen from 20·8 to 41 per cent, just as he had effected a partial separation of oxygen from air by the slightly greater diffusion velocity of nitrogen. The phenomena of diffusion and penetration of india-rubber are, however, widely different, for the rate of passage through the colloid depends on the facility with which the gas is capable of being liquefied within its pores. MM.

Deville and Troost discovered that certain gases penetrated tubes of iron and platinum, and this fact led Mr. Graham to enquire whether the penetration was not preceded by an absorption of the gas. His results proved that palladium, platinum, and iron did absorb hydrogen. Palladium, for instance, absorbs nearly 1000 times its volume of the gas, and most metals appear to have the power of selecting one or more gases in virtue of their colloidal character; for crystalline metals, such as osmium and iridium, do not occlude gas at all. The absorption of hydrogen by palladium led him to conclude that the gas was condensed into the metallic state; but, although much evidence was adduced, it may be doubted whether the metallic character of hydrogen was established.

Under the second classification of the papers, the research on "Arseniates, Phosphates, and Modifications of Phosphoric Acid," occupies a prominent place. It commenced with the explanatory hypothesis that phosphoric acid is disposed to unite with 3 atoms of base, and he then traced the method by which the metaphosphates and pyrophosphates were produced. It is impossible to overrate the importance of this research, for on it the theory of the polybasity of acids mainly rested.

The diffusion of gases appears to have led to the study of the diffusion of liquids, and in 1849 the splendid monograph on the latter subject was published, the results of which showed that diffusion supplied the densities of a "new kind of molecules," for in liquid diffusion we appear to deal no longer with chemical equivalents or Daltonian atoms, but with masses even more simply related as to weight. By continuing the investigation he was enabled to divide various soluble substances into crystalloids and colloids, the former having a rapid diffusion rate and the latter being marked by low diffusibility. He pointed out that, although chemically inert, in the ordinary sense, colloids possess a compensating activity of their own, arising out of their physical properties. The colloid is, in fact, the dynamic state of matter, the crystalloid being the statical condition. The colloid possesses *energy*, and it may be looked upon as the primary source of the force appearing in the phenomena of vitality.

His paper on "Speculative Ideas concerning the Constitution of Matter" will always be viewed with special interest, as it contained several remarkable expressions *of belief*, such as the suggestion that the various kinds of matter recognised as different elementary substances may possess one and the same ultimate or atomic molecule existing in different conditions of movement. With the atom at rest, the uniformity of matter would be perfect: but it always possesses motion due to a primordial impulse, and, as differences in the amount of this motion occasion differences of volume, matter only differs by being lighter or denser matter. The gaseous molecule is composed of a group of the preceding inferior atoms, following similar laws, and is thus

a reproduction of the inferior atom on a higher scale. Chemical combination consists in equal volumes of the different forms of matter coalescing and forming a new atomic molecule, and is therefore directly an affair of weight; and the combining weights differ because the densities, atomic and molecular, differ. And he points out that liquefaction or solidification may not therefore involve the suppression of the atomic or molecular movement, but only the restriction of its range.

All Mr. Graham's results were obtained by the simplest possible means, and it is not a little remarkable that throughout the forty-six papers which the volume contains but few plates and woodcuts are necessary for their complete illustration. All his apparatus which could be found was exhibited at the Loan Collection of Apparatus held during the past year at South Kensington Museum, where it excited much interest from its simplicity as compared with the complicated appliances there gathered together. Some remarks which concluded a description of it may not be out of place here, for it taught that although in certain researches, or for accurate observation and measurement, delicate and complicated instruments may be necessary, the simplest appliances in the hands of a man of genius may yield the most important results. Thus with a glass tube and a plug of plaster-of-paris Mr. Graham discovered and verified the law of diffusion of gases. With a tobacco-pipe he proved indisputably that air is a mechanical mixture of its constituent gases. With a tambourine and a basin of water he divided bodies into crystalloids and colloids, and obtained rock crystal and red oxide of iron soluble in water. By the expansion of a palladium wire he did much to prove that hydrogen is a white metal. And, finally, with a child's india-rubber balloon filled with carbonic acid he separated oxygen from air, and established points the importance of which from a physiological point of view it is impossible to overrate.

Dr. Smith has devoted 29 pages of the volume to a very accurate and valuable analysis of the papers, which he has in many cases collected from scientific periodicals now almost inaccessible even in this country. He has also added an excellent preface entitled "Graham and Other Atomists," which it may be well to quote at some length.

"Atoms and eternal motion are among the first-known scientific ideas. We find them discussed with full keenness by the earliest Greeks of whom we have received definite accounts." Dr. Smith then states the views which have been held from the time of Leucippus, "to whom the action of the atom as one substance, taking various forms by combinations unlimited, was enough to account for all the phenomena of the world." Graham took a similar view, and advanced the idea (of atomic motion with unity of material) to its utmost limit. The Greek told us all was motion; Graham considered that the diversity in motion was only the basis of the diversity of the material, or, in other words, that an atom constituted an element of a special kind ac-

cording to its rate or the peculiarity of its movements. Lucretius, as M. A. Ditte recently observed,* "a exposée la theorie atomique avec une vivacité, un éclat, une éloquence qui font du poëme de la nature un des chefs-d'œuvre de la littérature latine;" and Dr. Smith considers that this philosopher was the only full expositor of the doctrine among the ancients. Newton's well-known words are quoted, as they form an era in the theory of atoms, not so much by new ideas as by distinctness, and because they eliminate much confusion. Newton held that the primitive particles of which matter is composed are incomparably hard and incapable of wear, for otherwise water and earth composed of old worn particles would not be of the same nature and texture now with water and earth composed of entire particles in the beginning, and therefore, that nature may be lasting, the changes of corporeal things are to be placed only in the various separations and new associations and motions of these permanent particles. The first distinct attempts to define precisely the motion of gaseous molecules, and not of atoms, are by D. Bernoulli, who says—"The chief peculiarities of fluids are these: 1st, they are heavy; 2nd, they expand in all directions unless they are confined; and 3rd, they allow themselves to be compressed more and more, according to the increased force employed." Speaking of a vessel of air with a weighted cover, he says—"So the minute bodies, whilst they impinge on the cover, keep it up by their continually-repeated strokes, &c." The views of Davy, Rumford, and Herapath are then given, and it is pointed out that Joule investigated experimentally the hypothesis that gases consist of small bodies continually impinging on one another, their elasticity increasing with the temperature, the pressure of the gas being due to the impact of the particles against any surface presented to them. Dr. Smith shows that it was the object of Graham's life to find out what the motion of the atom was, and claims him to be "as strict an atomist as perhaps can be found," and it is certain that he did believe in the existence of atoms, but few minds have been more free from unconscious bias: we are convinced that he would have abandoned the atomic theory without reluctance if he had found a better to replace it.

We may quote, as it will be familiar to most readers, a paragraph just written by Mr. Gladstone,† which will aid us in explaining Mr. Graham's mental attitude. The learned writer points out that "even in the days of Bacon, even in the days of Dante, when knowledge, as the word was commonly understood, was so limited that some elect minds of uncommon capacity and

* "Constitution de la Matière," par M. ALFRED DITTE. Ann. Chim. et Phys., Feb., 1877, p. 151.

† On the Influence of Authority in Matters of Opinion. By the Rt. Hon. W. E. GLADSTONE, M.P.

vigour could grasp the whole mass of it, they still depended largely upon authority. For that aggregate of knowledge, which they were able to grasp, was but book knowledge and not source knowledge." It is hardly too much to say that Mr. Graham had grasped, early in his career, all that was then known of chemistry and physics. He cared but little for the prevailing theories of his time, but for all "source knowledge" derived from experiment he had the highest respect. In his first paper (in 1826) he alludes to the researches of "that ingenious chemist Mr. Faraday," and he constantly quotes the labours of others or makes their work a starting-point for fresh discoveries. No man ever had a higher reverence for authority or more constantly appealed to it.

SCIENTIFIC NOTES.

GEOLOGY, MINING, MINERALOGY, AND METALLURGY. — We have from time to time felt it our duty to draw attention to the services rendered to science by the American Government by means of the admirable Geological Survey of the Territories now being carried on at its expense and under its direction. It is therefore the more satisfactory that the British Empire is not behind the labours of a similar class. The Geological Survey of India may not include any notice of the living flora and fauna of that country, so rich in both departments of organic life, but the account of the fossil remains is given on a most magnificent scale. This will be at once apparent, when we mention that the issues before us, comprehending sixty large and well-executed plates, with the accompanying descriptions, is entirely devoted to one nook of the country, the province of Kutch, and to one group of Mollusca, the Cephalopoda, formerly known as ammonites. The value of documents, so full and so trustworthy, cannot be easily over-estimated. Our readers will regret to learn that Dr. Waagen, on whom has devolved the task of determining, classifying, and describing the specimens discovered, has been compelled, by ill-health, to give in his resignation, and return to Europe.

Dr. E. A. Smith's "Report of Progress for 1875 of the Geological Survey of Alabama" contains a general outline of the geological formation in the State, a history of coal-mining in Alabama since 1853, with notices of the features of the fields, of the character of the coals, and of the fossil vegetation. Copper ore exists in the metamorphic region, and is worked to a small extent. The ores are yellow sulphide, averaging some 10 per cent. of metal, also azurite and malachite. There is also a preliminary notice on the cotton-worm, the larva of *Aletia argillacea*, a small moth belonging to the Noctuidæ. The use of the arsenical compounds of copper is proposed for its destruction,—a method, as it appears to us, questionable on grounds of public health.

An account of the fossil organic remains discovered during the Geological Survey of the Province of Victoria, and used for the determination of the ages of the different geological formations of the region, is being issued in numbers of ten plates each, with corresponding descriptions, on the plan of the decades of the Geological Surveys of England, Canada, India, &c. The first plate is devoted to the skull of *Thylacoleo carnifex* (Owen). Much interest attaches to this extinct species from the controversies which have been waged concerning its nature and habits. Prof. Owen named it the Marsupial Lion, from the general resemblance of its teeth to those of the lion, indicating, in his opinion, its decidedly predaceous character. Dr. Falconer, Mr. Flower, and others, took a different view, and considered it as a harmless plant-eater, since a præmolar of a sharp-edged compressed form, like the carnassial of *Thylacoleo* is found in the still existing rat-kangaroos (*Hypsi-prymnus*). As Mr. F. McCoy, however, points out, they overlooked the fact that the *Hypsi-prymni* have behind the compressed præmolar a series of grinders of the ordinary vegetarian type, whilst in the marsupial lion all the teeth are of the true carnivorous type. Mr. McCoy considers that his Victorian specimen presents important differences from the New South Wales examples described by Prof. Owen, and that the species of the two provinces are really distinct. He would retain for the Victorian animal the name of *Thylacoleo carnifex*, and proposes that of *T. Oweni* for the New South Wales species.

The third report, since the re-modelling of the Mining Department of he Geological Survey of Victoria, has been issued. It forms a quarto

volume of over three hundred pages, more than a third of which is taken up by Mr. Brough Smyth's report on the progress made in the Geological Survey of Victoria during the year ending September 30th, 1875. Since the re-organisation of the department nearly 10,000 square miles of country have been surveyed and mapped on different scales, varying from one-eighth of an inch to 2 inches per mile, the latter being the scale of the Geological Survey, a much more convenient scale, by the way, for working with than our own. Besides this, nearly 3000 square miles are being surveyed and mapped on scales varying from half an inch to 2 inches to the mile. Perhaps the most important work done during the year is the publication of a new and corrected edition of the Geographical Scotch Map of Victoria on the scale of sixteen miles to the inch, representing all that is known of the geology of that country, and a first sketch of a geological map of the whole of Australia on the scale of 110 miles to the inch, in which are given the results of all explorations respecting which information is available, from the first discovery of the Continent up to the present time.

Mr. Krause's "Report on the Ararat Gold Field" gives the history of the discovery and progress of one of the most celebrated of our modern El Dorados, his information being derived from the *vivâ voce* accounts of the few surviving pioneers of this gold field. The first "rush" was made in 1855, and before many months a town built of canvas, wood, and even stone, sprang up, which gave shelter to upwards of 3,000 souls. A bye-road leads past the place to-day, and nothing remains to indicate the former bustling crowd but a few ruined stone huts, themselves almost hidden by a thick forest of young gum saplings sixteen or seventeen years old.

Mr. Reginald Murray's "Report on South Western Gippsland" shows that he has completed a Geological Sketch Map of the district, which extends over 3500 square miles. Mr. Howitt has done the same for North Gippsland, and the two reports form a most valuable addition to our knowledge of the geological structure and mineral resources of this interesting portion of the Australian Continent. Other reports follow on the Stawell Goldfields and the Kilcunda and Patterson Coalfields.

The question of the existence of gold in solution in the saline waters of mines has received a large amount of attention from the chemists of the Department. The employment of native pyrites as a source of sulphur in the manufacture of sulphuric acid has also received a large amount of attention, but as nearly all the pyrites found in Victoria contain arsenic in notable proportions, the Melbourne sulphuric acid manufacturers prefer to import native sulphur. The part played by particles of organic matter in the formation of nuggets has also received attention. A solution of gold terchloride containing a piece of rough metallic gold was found by Mr. Daintree, one of the chemists of the Department, to have become perfectly colourless. On further examination a piece of cork was found floating on the surface, and the piece of gold had so much increased in size that it would no longer pass through the neck of the bottle. The experiment was repeated with pieces of hammered gold, without success, but whenever the surface of the metal was roughened, deposition always took place.

Judging from the "Mineral Statistics of Victoria" for 1875, Nos. 11 and 12, alluvial gold mining has long ago seen its last days in Victoria, having fallen from a million ounces in 1868 to a little over 400,000 ounces in 1875. Quartz gold mining seems to be also on the decline, there being a steady decrease of 50,000 ounces between the returns of 1872 and 1875, the falling off from the previous year being over 22,000 ounces. The number of miners, too, is diminishing, having fallen from 58,000 in 1871 to 41,000 in 1875, the alluvial miners decreasing the more rapidly of the two. The Chinese miners are following suit, but this may be regarded as a blessing. The average earning of each man, however, is on the increase, the figures in 1875 being £104 as compared with £93 in 1871, showing that improvements have taken place in the mode of working. The deepest quartz mine is at Pleasant Creek, the lowest working being 1706 feet below the surface, proving that gold quartz can be worked

profitably at great depths. The total quantity of gold exported and minted in the colony from the date of the first d scovery to December 31st, 1875 is 45½ millions of ounces, valued at 182 millions of pounds sterling. Tin ranks next for £333,870, after which comes antimony for £104,865. Silver only shows for £14,617, copper for £8331, lead for £4700, iron for £2101, and coal for £8233, for the whole period to the end of 1875, showing that the mineral wealth of Victoria is to be found almost entirely in its gold-mines. Not a single ton of coal was either raised or exported during the year 1875, the imports of that article amounting to 214,000 tons.

"The Reports of the Mining Surveyors and Registrars for the Quarter ending March 31st, 1876," give the gold-mining statistics of the colony for the first quarter of the present year, and show the yield of gold from parcels of quartz raised during the quarter from some of the deepest mines in Victoria, the depth of the shaft &c., the estimated yield and quantity extracted, the gold received at and issued from the Royal Mint, the yield of gold from quartz and tailings, as well as from washing operations, and the number and distribution of miners on the gold-fields. The coin issued from the Melbourne Board of the Royal Mint during the quarter has been 124,046·53 ozs., besides 1,100·914 ozs. of gold bullion.

We have received an interesting little *brochure* on the minerals of New South Wales from the pen of Prof. Liversidge, who holds the chair of mineralogy in the University of Sydney. The author gives additional description of the minerals found in New South Wales, with their chemical composition and the localities in which they are found. In the account of the different gold-fields an important statement is made with respect to the first discovery of gold in Australia, which is worthy of record. It seems that, according to the "Evening News," of Sydney, for August 7th, 1875, that gold was discovered and officially reported to the Government of the day on February 16th, 1823, by the Assistant Surveyor, James McBrian, at a spot on the Fish River, about fifteen miles east of Bathurst. Mr. McBrian's field book is preserved in the Surveyor General's office, and contains the following entry:—"February 16th, 1823. At 81·50 to river and marked gum tree. At this place I found numerous particles of gold in the sand and on the hills convenient to the river." The use of the word "convenient" in conjunction with the worthy Assistant-Surveyor's patronymic seems to show that the first discoverer of gold in Australia was an Irishman. The knowledge of the discovery seems to have been suppressed, owing to the peculiar social condition of the colony at the time. The same thing was done when Count Stozelecki, in 1839, and the Rev. W. B. Clark exhibited specimens of gold to members of the Legislature of those periods.

Mr. Frederick Field, F.R.S., thus describes a Cornish mineral which is quite new to mineralogical science. "Its crystallisation appears to be rhombic; it is transparent and brilliant, of a clear green colour; hardness about 3·5. It is perfectly soluble in dilute hydrochloric acid, forming a nearly colourless solution. On heating a little over 100° C., the crystals lose water, and at a low red heat at once become brilliantly black. They decrepitate strongly. Analysis proved the existence of phosphoric acid, ferrous oxide, and water in the proportions that would lead to the formula $3FeO,P_2O_5,4H_2O$. From the great scarcity of the mineral only small quantities can be obtained for analysis, and this brief description must be regarded merely as a preliminary note on a mineral of great beauty and interest."

A meteoric iron, very rich in nickel, found in the province of Santa Catarina (Brazil) has been analysed by MM. Guignet and G. Ozorio de Almeida. The specimen contained 36 per cent of nickel, and is free from chrome, cobalt, manganese, and copper; neither is it mixed with any earthy gangue.

In a paper on Russian platinum-ore from the Oural Mountains, Mr. Sergius Kern, of St. Petersburg, says that considerable quantities of platinum-ore are every year mined in Nishni-Tagil and Goroblagodatsky districts on the Oural Mountains. The platinum-ore of these districts contain notable quantities of

oreign metals of the platinum group, except ruthenium, which is only in traces found in these ores. He also gives analyses of some newly found platinum-ores near these districts. which may be of some interest to chemists studying the properties of the compounds of rare metals of the platinum group as the ore is sold at the Mint in St. Petersburg at a very moderate price.

MICROSCOPY.—The oxyhydrogen microscope has been greatly improved by Mr. John Browning. Those who are familiar with the old instrument will notice a great increase of defining power, and when a suitable surface is employed to receive the image, some of the coarser diatoms, such as *Arachnoidiscus* and *Isthmia* are not only fairly shown, but will bear being viewed with some amount of additional magnifying power. Great care has been taken in the arrangement of the condenser to prevent the object from being injured by the intense heat of the lime-light, and, with such success, that balsam-mounted slides can be used with safety. The object-holder is adapted to receive the ordinary 3×1 slide. This instrument will prove a great desideratum for lecture room and class demonstration, and for this purpose, when the object is not too opaque or of an unfavourable colour, and tolerably flat, it will be found all that could be wished. It works well upon a disc of from 4 to 5 feet in diameter.* Microscopists must not expect even of this improved instrument the resolving or defining power they have been accustomed to with their usual microscopes; the conditions are entirely different, and involve many constructive difficulties, and the lime-light is, after all, a feeble source of illumination where great magnifying power is required. The instrument is rather to be judged in comparison with the older forms of gas microscope, and, when so examined, will be found a great improvement on all its predecesrors.

The President of the Royal Microscopical Society, H. C. Sorby, Esq., F.R.S., in his Anniversary Address laid before the Society his observations on the structure of disintegrated rocks, such as occur in various sands, clays, &c. He was led to investigate the subject chiefiy by having undertaken to examine and describe the mineral constituents of the deep ocean deposits brought back by the "Challenger." For the results of these researches our readers must be referred to the original paper † as the subject cannot well be abridged; as, however, the processes employed in investigation may aid other observers, they are given at some length. When stratified rocks are sufficiently hard and consolidated to be made into thin and partially transparent sections, many facts may be better seen in slices cut perpendicular to the stratification than by attempting to disintegrate the rock and examine the detached particles. It would, however, often be difficult to prepare thin sections of many deposits, and it becomes necessary to study them in another manner. If the particles are firmly held together by calcic or ferrous carbonate, or by any of the oxides of iron, they may be set free by the action of cold dilute hydrochloric acid, or by a stronger hot solution: but if the rock is consolidated by means of silex, this cannot be accomplished. Violent mechanical crushing must be avoided, since it would give rise to false results by fracturing the constituent grains. Such an amount of crushing as can be effected with a small stiff brush made with bristles does, however, appear to be admissible, since it could scarcely break the separate fragments. A portion of the crushed material may then be sufficiently diluted with more water, placed on a glass slip with a projecting ledge, and covered with a piece of the usual thin glass. By this means the larger particles are seen separate, but the smaller have a great tendency to mass themselves together. Since the index of refraction of the various grains is in all cases considerably greater than water, the outline of even the most transparent constituents is well seen, but at the same time this difference in refractive power may make it impossible to study the internal structure or optical characters of the larger grains. This difficulty is overcome by mounting in Canada balsam, which has so nearly the same index of refraction as that of many of the constituent grains that, even when

* With a powerful electric light a disc of 7 or 8 feet diameter can be obtained.
† Monthly Microscopical Journal, vol. xvii., p. 113, March, 1877.

their outline is as irregular as possible, light passes through them almost as though they were thin slices with parallel polished surfaces. This enables us to study the external staining, internal structure, and optical characters to great advantage, since they are not interfered with by any dark shading due to the bending of the light out of the line of vision. When examined in water there is no difficulty in recognising extremely minute granules of the kaolin of clays, whereas, when mounted in balsam, they may be almost or quite invisible, but this circumstance is of great advantage in observing certain facts, since, by making them invisible, other objects may be distictly seen which otherwise would be completely hidden by the surrounding granules.

Mr. Sorby mounts loose sandy deposits in the following manner:—Having placed a very small quantity of dissolved gum on the glass plate, the requisite amount of the deposit is taken and mixed with the gum, and sufficient water to make it easy to separate the grains and spread them uniformly over the space, which will be afterwards covered with the thin glass. The water is then allowed to evaporate slowly, and though much of the gum collects round the margin, by properly regulating the quantity originally added, enough remains under the larger grains to hold them so fast that they are not squeezed out with the excess of balsam. More gum than is sufficient for this purpose should not be used, since it may make itself too conspicuous on the object. When the proper quantity has been used its presence can be detected only at the under-surface of the grains, and in that situation does not in any way interfere with the study of the object. This method prevents the grains from settling to one side of the object, even when soft balsam is used, which is desirable, since it penetrates more completely at a lower temperature into irregularities of the surface, and into the interior of compound grains. In these observations, the employment of object glasses of large angular aperture is disadvantageous, and with the higher powers it is impossible to focus down into the interior of grains of sand so as to view their minute fluid cavities. For these researches Mr. Sorby has had an eighth of only 75° of aperture constructed by Messrs. Beck, with which all parts of the object can easily be reached and perfect definition obtained with a power of about 600 linear; the form of grains as small as $\frac{1}{1000}$ of an inch in diameter can also be seen. In the case of very fine transparent particles of such substances as pumice, the form is best seen when the illuminating pencil is rendered divergent by means of a conclave lens, instead of employing the usual condenser. This mode is, however, only available for the lower powers, on account of the light being somewhat feeble. A considerable portion of the paper is occupied with detailed information respecting the examination of various fragmentary rocks and the means employed for their identification. Considerable use is made of the polariscope, as in the examination of rock sections. The paper is full of matter of the utmost value to the micro-mineralogist and geologist.

Light.—M. Bert has undertaken some new experiments upon the influence of different colours upon vegetation. These experiments, performed chiefly upon the sensitive plant, lead to the following results:—Green light kills plants; plants submitted to the influence of the green ray die in a short time. Under the influence of the red rays the sprays become elongated; the leaflets are raised so as to form a smaller angle with the branch than in the normal state; the plant appears to become etiolated and yet it remains alive. Under the influence of the blue rays the process is reversed, the leaflets become perpendicular to the branch, whilst in white light an intermediate position is maintained, *i.e.*, the leaflets form with the branch an angle of 45° on one side and of 75° on the other. M. Bert explains these facts as follows:—At the level of the point of attachment of the leaflet there is a motor enlargement, which increases or lessens in force according to the different kinds of rays. Under the influence of red rays there is formed in these enlargements a particular substance, osmotic, and capable of attracting water. This substance generally disappears under the influence of blue rays. If we place it under a glass shade, red on one side and green on the other, the plant turns its leaflets towards the green, that is to say, towards the colour which kills it, and in fact it dies.

ELECTRICITY.—A new electric lamp designed by M. P. Jabloschkoff has been described to the Academie des Sciences by M. L. Denayrouse. The new source of light is composed of two pieces of charcoal fixed in a parallel position at a little distance from each other, and separated by an insulating substance capable of wasting away at the same speed as the charcoal. When the electric current begins to pass the voltaic arc is formed between the two uncovered extremities of the two charcoals. The nearest layer of the insulating matter melts, is volatilised, and slowly lays bear the two rods of charcoal just as the wax of a candle progressively uncovers its wick as the combustion is propagated downwards. The heat springing from the combustion of the charcoal is utilised for the fusion and volatilisation of the insulating mixture. The composition of this latter may be varied indefinitely, since most earthy matters may be employed. The simplest mixture provisionally adopted consists of sand and powdered glass, which, with an equal electric power, gives double the light of a regulator. The author has been able to divide the light produced by a single source of the current. With a single Gramme machine of the common make he has caused three sets of charcoals to burn at once.

We learn from the "Polytechnic Review" that, in order that ordinary fireproof safes should be in a measure rendered burglar proof, the Louis Reutzsch Manufacturing Co., of Meissen, has constructed a wire covering, which is placed in an electrical circuit with an alarm bell. If any of the wires forming the cover be cut, which must necessarily happen before the safe can be opened by an intruder, the circuit is broken and the alarm bell is sounded. The device is likewise applicable to doors and windows.

Mr. Alexander Bain, the inventor of the electro-chemical telegraph, died in January last in the Home for Incurables, near Kirkintilloch, of which institution he had been an inmate for several years.

Mr. Alfred Smee, F.R.S., &c., the inventor of Smee's battery, also died in January last. Mr. Smee was the author of "Elements of Electro-Metallurgy," "Elements of Electro-Biology," and several other scientific works.

TECHNOLOGY.—Oenokrine is the name of a test-paper sold in Paris for the purpose of detecting the fraudulent colouration of wines. With a genuine red wine the colour produced is a greyish blue, which becomes lead-coloured on drying. With magenta and other aniline colours it turns a carmine-red; with ammoniacal cochineal, a pale violet; with elder berries, the petals of roses, &c., a green; with logwood and Brazil wood, the colour of dregs of wine; with Fernambucca wood and phytolacca, a dirty yellow; with extract of indigo, a deep blue. The manipulation required is very simple. A slip of the paper is steeped in pure wine for about five seconds, briskly shaken in order to remove the excess of liquid, and then placed on a sheet of white paper to serve as a standard. A second slip of the test-paper is then steeped in the suspected wine in the same manner and laid beside the former. It is asserted that 1-100,000th of magenta is sufficient to give the paper a violet shade, whilst a larger quantity produces a carmine-red. The inventors of the test-paper, MM. Lainville and Roy, are also said to have discovered a method of removing magenta from wines without injuring their quality, a fact of some importance if it be true that several hundred thousand hectolitres of wine sophisticated with magenta are in the hands of merchants.

In a letter written from Mentone to the Manchester Literary and Philosophical Society, Mr. Joseph Sidebotham, F.R.A.S., says:—"My attention has been for some time directed to the growing use of the aniline colours for tinting photographs. Now I find they are being *extensively* used in paintings and water-colour drawings, and the colours regularly sold for that purpose. Anyone who knows the speedy alteration by light of nearly all of these colours will protest against their use, and a statement of this with the authority of some of our chemists would probably have the effect of causing them to be discontinued by all artists who care to think that their works should last more than a single year."

XUM

Mr. A. F. Taylor, of Andover, Mass., sends the following note on poisonous india-rubber toys to the "Boston Journal of Chemistry:"—"Prof. B. Tollens, in the "Journal of the Berlin Chemical Society," of November 13, 1876, calls attention to the injuriousness of many of the articles manufactured from caoutchouc, which, among other impurities, contains a very large per cent of zinc oxide. In the rubber nipples of milk-bottles for children this has often been found to be the case, and so much attention has been called to this fact that the manufacture of these nipples containing zinc oxide has to a great extent ceased. But more recently suspicions have been aroused concerning the quality of children's toys, dolls, animals, &c., made from rubber. One case, in which a child, having one of these dolls, had had it for some time in its mouth, grew sick, and the doll, laid in vinegar, became covered with an incrustation (without doubt zinc acetate), led to direct investigation. In 0·7325 grm. of such a doll, 0·4446 grm. zinc oxide was found, or 60·58 per cent. Another portion gave, after being subjected to a red-heat, 62·64 grms. of ash, yellow while hot, white on cooling. In the ash besides the zinc were traces of lime, iron, and phosphoric acid. From another doll which had been warranted 'harmless' 57·68 per cent of ash was obtained, consisting almost wholly of zinc oxide."

The following gentlemen were requested by the Council of the Royal Dublin Society to consider and report upon the scientific prospects of that Society:—Sir Richard Griffith, Bart., F.R.SS. L. and E.; H. Lloyd, D.D., F.R.SS., L. and E., Earl of Rosse, F.R.S.; M. H. Close, M.A.; W. F. Barrett, F.R.S.E.; W. R. M'Nab, M.D.; Gerald Molloy, D.D.; Alexander Carte, M.D.; Alexander M'Donnell, M.A., J. Emerson Reynolds, M.D., F.C.S.; Wm. Andrews; Edward Hull, M.A., F.R.S.; Alex. Macalister, M.D.; B. B. Stoney, M.A., M.I.C.E.; Lord Gough, Howard Grubb, M.E., F.R.A.S.; Robert S. Ball, LL.D., F.R.S.; Charles A. Cameron, M.D.; Robert M'Donnell, M.D., F.R.S.; G. Johnstone Stoney, M.A., F.R.S.; Charles Kelly, M.A., Q.C. From their report we learn that, while while the Royal Dublin Society during the century and a half of its existence has engaged in many most useful branches of scientific work, its labours in relation to some of these have been from time to time superseded owing to their being entrusted by the Government to Public Departments, in order to continue them on a larger scale than the Society could have attempted; and the Society's work has been thus from time to time circumscribed by the establishment of such institutions as the Ordnance Survey of Ireland, the Geological Survey of Ireland, and the Royal College of Science. It is now proposed that the Society's prolonged scientific labours in connexion with its Museums and Botanical Garden shall in like manner be undertaken by the Government. If the Royal Dublin Society assent to this change the Committee are of opinion that it will do a very important service both to science and to Ireland if it can mould itself into two societies, of which one shall be devoted to science, pure and applied, in all its branches, and the other to agriculture. It is recommended that the present members of the Royal Dublin Society be members of both these Societies, and that those existing members of the Royal Irish Academy who are not already members of the Royal Dublin Society shall have privileges with respect to the Scientific Society corresponding with those to be enjoyed by members of the Royal Dublin Society. It is further recommended that within the Scientific Society there be an inner body of Fellows.